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ORIGINAL ARTICLES.

THE TREATMENT OF PERITONITIS.

Read in the Section on Gynecology at the Thirty-ninth Annual Meeting of the American Medical Association, Cincinnati, May, 1888.

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PATHOLOGICAL DEFINITION OF PERITONITIS.

"Increased vascularity-softening or thickening of the peritoneal membrane, with effusion of coagulable lymph, or of a sero-albuminous or sero-puriform, or sero-sanguineous fluid, sometimes with organized adhesions." Copland.

When Dr. Rushton Parker asserts in his article on Peritonitis, published in the "Dictionary of Practical Surgery," that "the treatment of peritonitis is simple enough whatever be its cause," and that "opium, alcohol and liquid diet is the treatment that may be uniformly adopted in all its forms," he fails to apprehend that it is only through a perfect diagnosis in each case that we can see in what direction surgical or therapeutical interference should be attempted; for connected with the treatment of peritonitis are several questions I will submit for your consideration:

Is acute peritonitis ever idiopathic?

Is it a symptom or a disease?

Is it always septic?

Again I may ask—

Does the treatment properly belong to the physician or surgeon?

In your answer you will be brought irresistibly back to diagnosis, since from it all treatment must flow. It is then of primary importance that in every case the ætiology be definitely settled at the earliest moment. When we have arrived at the conclusion that peritonitis is present or impending, and we have discovered the cause, the blow must be struck simultaneously with the advent of the enemy; no delay can safely be tolerated, the only hope of rescue being the sudden arrest of the disease. We must learn the beginnings. As remarked by Sir William Gull: "Often when the gathered clouds of the final storm have filled the atmosphere it is in vain that we look around to see from what point of the Heavens it began." By the time that the normal outlines of the abdomen are obscured by tympanitic distension, respiration quickened and shallow, the pulse rapid and wiry, the supreme moment for precise diagnosis is past.

What are the grounds for a true discrimination? Certainly not the teaching that acute peritonitis may be idiopathic in a healthy subject. Upon this subject Dr. Savage, of Birmingham, England, in 1885, expressed the following opinion: "We are learning, if we have not already learned, to look upon peritonitis as a symptom of some organic change and not as a disease in itself; and this is well for our patients, because operative measures can do much for it; we shall ere long regard so-called 'idiopathic peritonitis' almost as a curiosity." Professor Bartholow's opinion is, that the great majority of cases of peritonitis come from previous disease in the peritoneal or pelvic cavity. He believes it to be extremely rare, indeed, for an idiopathic case to occur. Habershorn refers to 3,752 inspections made in Guy's Hospital; 501 were instances of peritonitis, but could not find a single case that he considered idiopathic. "A pure idiopathic peritonitis is difficult to realize under any circumstances; in puerpera it is impossible." Barnes.

Believing as I do, that acute peritonitis is never idiopathic but must be septic I am compelled to believe that the treatment must be modified in accordance with these views.

THE EVOLUTION OF THE TREATMENT OF PERITONITIS AFTER LAPAROTOMY.

On January 1, 1859, Sir Spencer Wells made an abdominal section and removed an ovarian tumor; the patient died thirty-two hours after. One hour after the operation she had a pulse of 100, and complained of some pain; a suppository containing ⅓ grain of morphine was passed into the rectum; two other suppositories at intervals of half an hour; a sixth suppository at midnight; pulse 130; 9 A.M., pulse 124; eighth suppository at noon. The autopsy revealed the presence of peritonitis. A considerable amount of free liquid was present in the cavity generally; the fluid had a pungent, irritating effect upon the skin. Sir Spencer in reviewing the case asks the pertinent questions:

What was the cause of death?

Did she get too much opium? Two grains of

morphine within ten hours and 1 grain in the succeeding twelve hours. He dismisses this consideration and asks "Did she die of peritonitis?" This question he answers thus: "My impression is, that if peritonitis killed her, it was indirectly by the formation of a morbid poison; the serum was very acrid." "If then my patient could generate a poison capable of killing other people may it not have killed her?" His practical commentary is in these words: "It may possibly be advisable, in some cases, to provide for a free outlet of the effused serum." (It was about this period when Baker Brown quailed before what he called peritonitis as a cause of death after ovariotomy.) These reflections were followed by the introduction of the drainage-tube and drainage of the peritoneal cavity.

Sir James Paget said in 1862, in an address delivered before the British Medical Association, that "some of the deaths after surgical operation were preventable; and that "the mortality will be reduced if the members of the Association will decide that it shall be."

In 1864, Sir Spencer Wells directed attention to the existence of germs in the atmosphere, and advised precautions against their entrance from without or their development within, in abdomdominal sections. In 1877 Lister arrived in London, and the questions of aseptic and antiseptic surgery, and their applicability to ovariotomy were considered; for in the inflammatory products within the peritoneal cavity, the effusion, germs were found; living organisms with an indefinite power of reproduction, and when once implanted in a suitable soil multiplying indefinitely. This is the doctrine of the day.

Germs are living. All living things require certain conditions for their growth and development; vitality their poison, non-vitality their food. We do not propose to enter into the question, whether microörganisms be the cause or result of the morbid processes with which they are associated, but to study the methods of interference: 1. By removing the germs; 2. By removing the soil in which they grow.

Microörganisms outside the body, can be destroyed by germicides; but when they have once gained entrance and are inside the pelvic cavity all we can do is to sustain the organism, in the struggle for existence between the cells which compose it and the bacteria which have invaded it. The clinical means most highly valued are opium by the physician; salines drainage and surgical procedures, when necessary, by the surgeon.

Grailly Hewitt, of London, in 1868, quotes Mr. Clay as saying:

"The chief mortality, 48 per cent., arises from peritonitis after ovariotomy." Baker Brown after a series of disasters utters the truism: "It's the peritonitis that beats us." Tait now informs us that now we beat the peritonitis; on the slightest indication of its appearance, after ovariotomy, we give a rapidly acting purgative, it matters not what, the patient's bowels are moved, and the peritonitis disappears." "This practice," says Tait, "was introduced by me in 1875, and is now almost uniformly adopted." He continues: "How different from the views we had drilled into us years ago, that opium was the sheet anchor of the practitioner in all abdominal troubles, when I say that all opiates are forbidden in my practice."

This may be regarded as the latest phase in the evolution of the treatment of peritonitis after abdominal sections. This I regard as the greatest advance that has been made, and to Mr. Tait is solely due the credit of it, In reply to a letter of recent date he writes as follows:

7, The Crescent, Birmingham, April 16, 1888.

Dear Sir:—Thanks for your very kind letter. I am perfectly satisfied, and I think the majority of practitioners who have tried it also agree that the best treatment for peritonitis, especially when occurring after an operation, is an early administration of saline cathartics. It is a practice which I had not seen the slightest reason to depart from. Yours very truly,

LAWSON TAIT.

In a letter from Dr. Joseph Price, of Philadelphia, occurs the following statement with reference to the treatment of acute peritonitis: "I hold very strong convictions in the strictly good effects produced by the free use of salines; they do most decidedly imperil the safety of the many germs, beside relieving the overloaded and inflamed tissues and congested conditions of all the abdominal and pelvic viscera. I have not the power of too strongly urging the use of salines upon the slightest indications of local or general peritonitis. I have never known them to disturb in any way the vital power other than to benefit."

Dr. Charles B. Penrose, of Philadelphia, sent the following reply to a letter requesting his experience in the treatment of acute peritonitis.

"My experience is limited altogether to peritonitis following laparotomy; in all cases of abdominal section, sulphate of magnesia or Rochelle salts is administered in a large dose the moment any tendency to undue distension or general abdominal pain is noticed. I think that when patients survive the first shock of peritonitis, they die subsequently from the same causes which produce death in intestinal obstruction from other causes. At the onset of the peritonitis it may be absorbed by salines; after it is well under way, however, I think that the salines save life by overcoming the intestinal obstruction. I think that statistics show beyond a doubt the superiority of the saline treatment over the opium treatment, at least after laparotomy." Speaking of the one hundred consecutive successful cases of laparotomy by Dr. Price and himself, he says he can-

not recall one case in which opium, even paregoric, was given.

Grieg Smith in his work upon "Abdominal Surgery," in the first edition, says: "After abdominal operations if peritonitis, local or general follow, opium is not the best treatment. As a rule, sedatives are to be condemned. I had oftener than once recourse to a saline purge in peritonitis; a purge carries off great quantities of gas and fluid, relieves the distension, and probably by its physiological action relieves engorgement of intestinal vessels."

Grieg Smith in the second edition of his work says: "Over and over again, I have been able to demonstrate to students and medical men, the value of a saline purge in case of incipient peritonitis. In grave abdominal cases I positively like to see diarrhœa natural or artificial. Slowly, but surely, the therapeutic virtues of purges in operation are being recognized. Among the most enthusiastic supporters of the plan are such well-known surgeons as Gill Wylie, Baldy, Penrose, and Gardner." Here he quotes the last named as saying: "In my work during the year I have given no opium, and invariably, immediately after the appearance of distension, pain, vomiting, I have given purgatives with the most signal advantage.

We are not so completely impressed with the authoritative decisions of tradition as to believe the rational treatment of acute peritonitis to be—to benumb the sensibilities, reduce the respirations, and contract the pupils with opium in heroic doses; rather should we prevent the presence of germs and expel them from the pelvic cavity by drainage.

Narcotics.—For opium, its advocates claim that pain is annulled, the nervous system tranquilized, sleep induced, the shock lessened and the peristaltic movements of the bowels arrested.

Salines.—For these, their advocates claim that by their use, we "imperil the germs," deplete congested peritoneal vessels, and drain off through the intestines serum contained in the peritoneal cavity.

While we admit that opium relieves pain, we also claim that it prevents the escape of poisonous products. We also deny that it is necessary to give opium to arrest peristalsis, for the reason that in an acute attack of peritonitis complete paralysis of the muscular walls of the intestines exists, destroying its function and producing often obstruction.

In support of this view, Godfee (as to the diagnosis in peritonitis) says: "It seems worth emphasizing that peritonitis *per se* ought always to be considered as one of the possible causes of intestinal obstruction, and that the complete absence of visible peristalsis is an important indication of its presence."

Charles Penrose, in an article published in the *Medical and Surgical Reporter*, Philadelphia, says: "The danger of moving the bowels after exposure of the peritoneum has been much exaggerated. It seems probable that the peritoneum is subjected to at least as much irritation from the tension and movement caused by the distending gas as it would be from any peristaltic motion of the muscles of the intestines."

We are advised to keep the bowels in splints; this is in direct conflict with the law that microorganisms require the condition of rest for their development; they will not multiply in the circulating blood. I had one painful experience where I kept the bowels in splints, after an ovariotomy; during the first week nothing untoward happened, but on the seventh day peritonitis supervened, until on the fourteenth day, when she died with symptoms of strangulation and the autopsy revealed that it depended upon adhesive bands, uniting coils of intestines. Had I prescribed a saturated solution of sulphate of magnesia and kept up a peristaltic action, the formation of false bands and adhesions could not have occurred. In confirmation I will refer to the *Berliner Klinische Wochenschrift*, January and February, 1874.

On the 28th of June, 1887, I made an abdominal section and removed both ovaries; they were cystic; on the second day following a chill and rapid development of peritonitis. I immediately gave half an ounce of saturated solution of sodæ et potass. This was followed by profuse watery discharges; the vomiting ceased; the tympanitis disappeared, the pulse and temperature declined in a few days becoming normal—complete recovery.

On the 13th of November, 1887, I was called to visit Mrs. C., æt. 36. I found a ventral hernia with strangulation; the tumor the size of the double fist; symptoms urgent. The taxis having failed laparotomy was decided upon, stricture divided reduction and excision of the sac; the peritoneal cavity closed by stitches, the final dressing as after ovariotomy. On the third day she had a severe chill, pain followed by vomiting, tympanitic, distension of the abdomen. Pulse, 120, temperature 101°. Treatment, heroic doses of opium prescribed by the attending physician. I ordered the opium discontinued and gave a full dose of magnesia; copious purgation followed, after which the peritonitis rapidly subsided and the patient speedily recovered.

Traube reports a case of a patient affected with general peritonitis, and in whom after death no adhesions or bands between the folds of intestines existed, although there were distinct evidences of false membranes on the parietal peritoneum. He attributes this unusual occurrence to the fact that the patient before death, had suffered from diarrhœa; he believes that constant peristaltic movements, which take place owing to intestinal

irritation, prevent the formation of adhesions and break those which may be formed. He accordingly questions the advisability of keeping the intestines at rest; claims that it facilitates the formation of adhesions; he is not convinced that inflammation is increased by the peristaltic movements, and suggests that peritonitis be treated by aperients.

While Treves advocates the use of opium in peritonitis, he admits that the clinical outlines of the case may be consequently blurred and serious errors in the diagnosis result in consequence.

THE DANGERS OF PERITONITIS.

The trinity of peritonitis, tympanitis and vomiting are the Furies of abdominal surgery.

In 1868, Baker Brown reported 111 cases of ovariotomy and thirty-five deaths. Speaking of the danger to be apprehended, places prominently, acute peritonitis as of most frequent occurrence; he gave opium in heroic doses, and finally says: "My experience has not given me that confidence in opium which most physicians possess." Peaslee, in 1872, in discussing peritonitis after ovariotomy said: "Give opium in quantities sufficient to remove the pain, and especially by hypodermic injection of muriate of morphine ¼ to ⅓ grains, every two hours or oftener.

In 1872, Sir J. Y. Simpson in a lecture on ovariotomy, said: "Perhaps the most frequent cause of death in connection with the operation is peritonitis. The treatment is opium."

Barnes in 1874, in his treatise upon this subject, recommends leeches, opium and mercury.

Thomas said: "Peritonitis, which proves the cause of death in about one-fourth of all who die from this operation, (ovariotomy) should be treated by full doses of opium.

In the International Encyclopedia of Surgery, 1886, we find the following: "After ovariotomy peritonitis is to be counteracted by increased doses of morphine hypodermically or by the rectum (Chas. Caurel Lee, New York).

In 1866, Dr. Gardner, of Canada, opened the peritoneal cavity thirty-eight times with three deaths, and he declares that, "experience has taught him that opium in most cases is more harmful than beneficial."

TREATMENT OF ACUTE PERITONITIS BY ABDOMINAL SECTION.

I believe that we ought to advocate and practice free incision drainages as practiced in the joints and in the inflammatory conditions of the pleura. The question of operative treatment in puerperal, as well as in other forms of peritonitis, has been strongly presented in a paper by Dr. Chalmers on Puerperal Septicæmia before the British Gynæcological Society.

Lawson Tait in a recent publication declares, "I have now come deliberately to the conclusion that it is an act of almost criminal omission to allow a case of peritonitis to die without abdominal section." Dr. Barnes has expressed similar views. Whenever pus has formed and collected in the abdomen and pelvis, he believed the indication was strong to make an incision so as to give it exit. This is in accordance with the rules of surgery in the brain, in the pleural cavity, and why not in the abdomen.

In a letter from Grieg Smith upon the surgical procedure, he writes as follows:

16 VICTORIA SQUARE, CLIFTON, April 6, 1888.

My Dear Sir:—In treating suppurative peritonitis by operations I am gradually coming to the belief that treatment with a wet and not a dry peritoneal cavity is advisable. The intestines are kept floating in a warm and mildly antiseptic fluid for one or two days, or even longer. I think it prevents the formation of adhesions, and consequent gaseous distension with its serious consequences.

J. GRIEG SMITH.

From the above quotations we are led to infer that opium is the routine remedy in acute peritonitis; is believed in as profoundly as if it were a religious dogma, and Professor Clark is referred to in the same spirit that actuated the Church of Rome in her appeal to the Greek Fathers to sustain her supremacy.

That the acceptance of an opinion in medicine does not afford evidence of its truth, is well illustrated by the fact that the sanguinary doctrines held by Professor Charles D. Meigs are now only found in his books, and not adopted as formerly by clinicians. It will be well to remember that the influence of authority in matters of opinion, does not prove that numerous persons have carefully examined the questions upon its own merits, and have founded their conclusions upon an independent investigation of the evidence. So the number of authorities adduced has, in fact, no weight, and thus their numbers alone are no more entitled to be considered as independent observers than the successive compilers who transcribe an historical error are entitled to be reckoned as independent witnesses.

DENTOGENY.

Read in the Section on Dental and Oral Surgery, at the Thirty-ninth Annual Meeting of the American Medical Association, May, 1888.

BY WILLIAM C. BRITTAN, M.D.,
OF DETROIT, MICH.

The first indication of tooth development in the human embryo is noticeable at a period somewhere about the fortieth day of fœtal life, and consists simply of an increased thickening of the epithelium over that part which is to form the future aveolar border.

At this time may be found, sometimes, not always, upon the lingual surface a slight depression or groove, the "dentinal groove" of writers, which when present consists of an infolding only of the mucous tissue, and is usually more marked

anteriorly than elsewhere. As, however, it is not always present, no histological importance can reasonably be attached to it. Later a change is seen to occur at certain points in the cells of the epithelium, consisting of an enlargement of the cells and their nuclei, these points corresponding nearly to the position of the future teeth. This changed epithelial structure dips down into the adjacent embryonic tissue in the form of "cords," or rather of tubular glands (Plate 1). The cells of the outer layer of these cords have a columnar form, yet they are seen to have now lost much of their resemblance to those of the columnar layer of the epithelium with which they are said to be identical. Plate 2 shows one of these cords highly magnified.

These epithelial cords constitute the embryonic enamel elements and later form what is known as the "enamel organ," which is concerned mainly —but not wholly—in the production of the enamel of the tooth. Coincidently with the above changes an increased activity has been in progress in the submucous tissue adjacent to these cords resulting in a multiplication of its cells and a consequent increased density of this tissue at points which occur sometimes beneath the cords, sometimes at one side (Plate 1–D.) and often one upon each side, in which case the enamel elements for two separate teeth of the same type are furnished from one cord.

These cell clusters of the submucous tissue gradually assume a papilliform appearance, pushing up against the overlying cord, in which they soon become nearly enveloped, and now constitute the so-called "dentine germ, or organ," In these two germs—the enamel and the dentine—we have the prime factors of tooth-building. And the processes here employed furnish problems many of which are yet unsolved. The interpretations which we shall give are solely the result of our own study of these tissues. The micrographic illustrations are from some of the preparations used for that purpose, and the preparations themselves are from the human embryo. If we err in any of the statements here made it is quite consoling to know that all other writers upon this subject have done the same. We believe, however, that we are well fortified.

Rapid developmental changes now occur. The cord, by a separation of its walls, assumes a somewhat "stirrup-like" form, its base conforming to the contour of the "dentine organ" until by the upward growth of the same all that part which is to compose the crown of the tooth is covered by the enamel organ in the form of a cap (Plate 3) which now separates itself from the original cord by the breaking up of that structure.

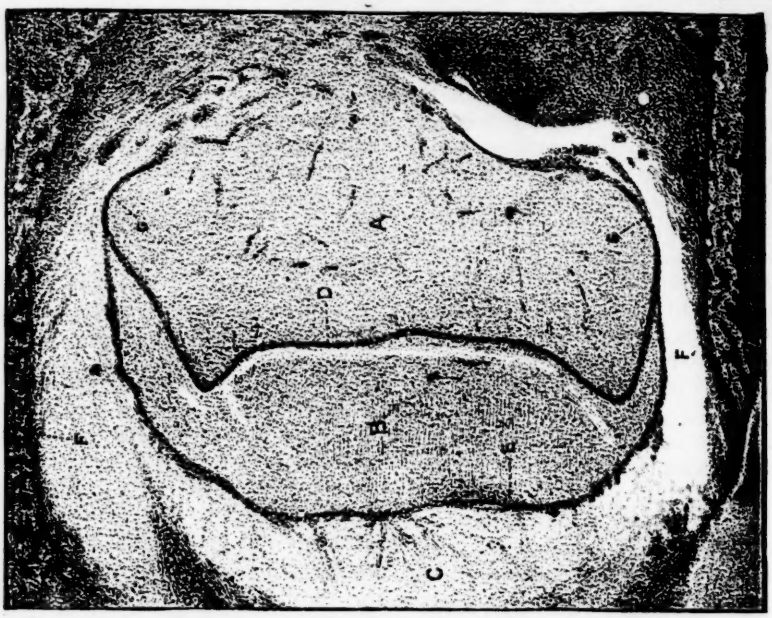
In this connection we would suggest that the prevailing dogma that the enamel organ for the second set of teeth is supplied from the cords of the first is incorrect. As a matter of fact they are derived directly from the mucous epithelial layer just as in the first instance.

All the above changes occur previous to deposit of either enamel or dentine, but we now have both these organs in a stage of development nearly sufficient to begin that work, and from this point the progress of development is much slower than previously.

We will now, by referring to Plate 3, note some of the changes that have taken place in the "enamel organ" in its transition from the cord with its outer wall of columnar cells to its present condition. By the separation of its walls in the course of its development the cells of its interior have by growth and separation, and attachment to one another by their processes, now assumed the form of stellate cells, and now constitute that part of the organ known as the "stellate reticulum (B), the exact function of which is not understood. This structure occupies all that part lying between the inner and outer investments, viz.: the wall in contact with the dentine organ (D) and that in contact with the saculus proper (E) which latter is the product of the contiguous embryonic tissue. Further, that at the point where the enamel organ embraces the *cervical portion of the embryonic tooth* (G) *the wall of the organ is folded upon itself*, thus bringing both walls (D and E) into mutual contact. These walls, although originally one and the same, here diverge, and now form two distinct and well differentiated structures. We also note that the outer investment (E) has lost all appearance of a columnar cell layer which characterized the original cord, and is now composed of a system of vessels and capillaries distributed through a fibrous membrane whose cells (mostly fusiform) now lie parallel to its surface.

At the inner wall (D) a very great change has also taken place. Here the original columnar form of the cells is still preserved at this stage of development, although greatly reduced in size, and having also lost other marked features of structure. Plate 4 gives a very good illustration of this boundary highly magnified.

The changes above mentioned as occurring in these two investments might with some show of reason be accounted for as due somewhat to mechanical influences. For instance, the outer wall (E—Plate 3) in assuming its convex form in which we now find it would, so to speak, be put upon the stretch. In consequence its cells would be forced into a horizontal position. On the other hand there would be a crowding together of the cells in assuming a concave form in the inner wall (D) thus maintaining their columnar form. These walls (D and E) or boundaries are said to be identical with the so-called "malpighian" layer of the epithelium, the columnar cells of which (D) are here transformed into "ameloblasts," or enamel forming cells.



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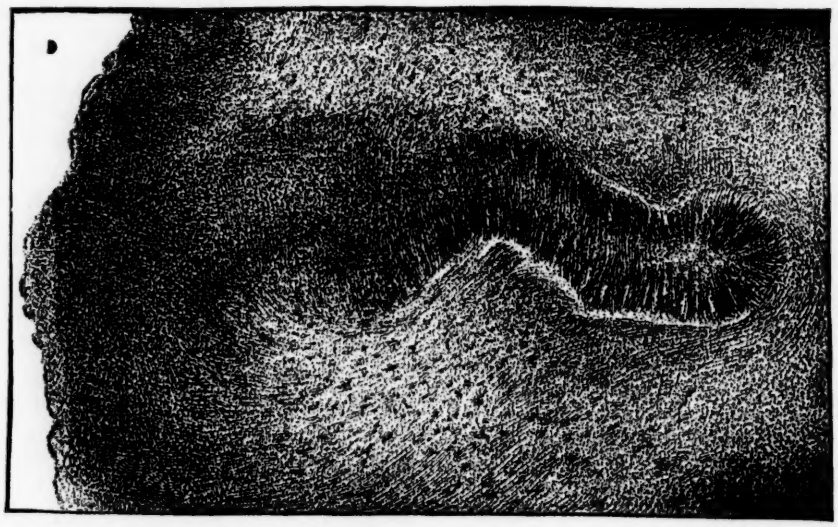

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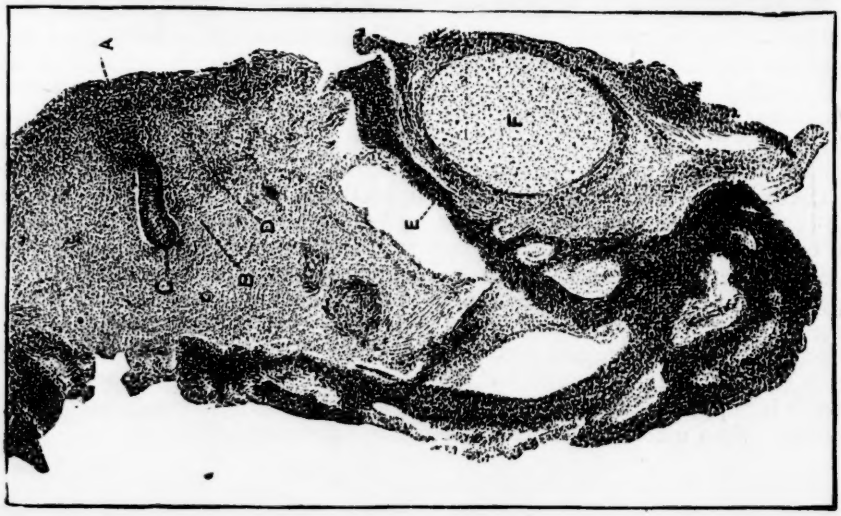

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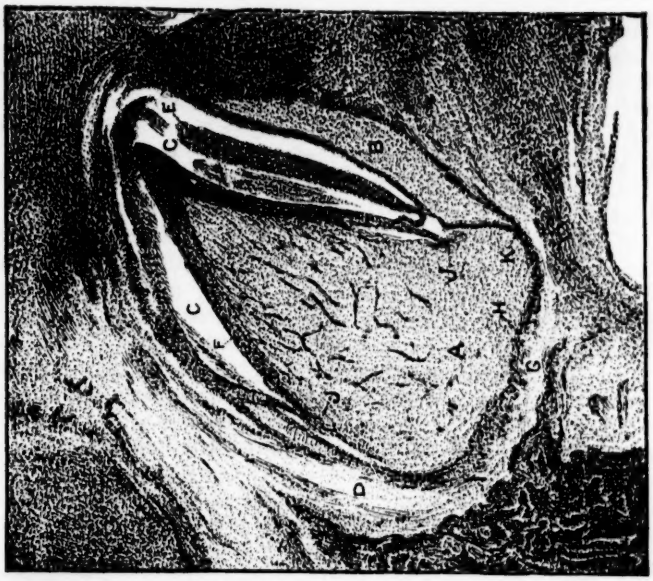

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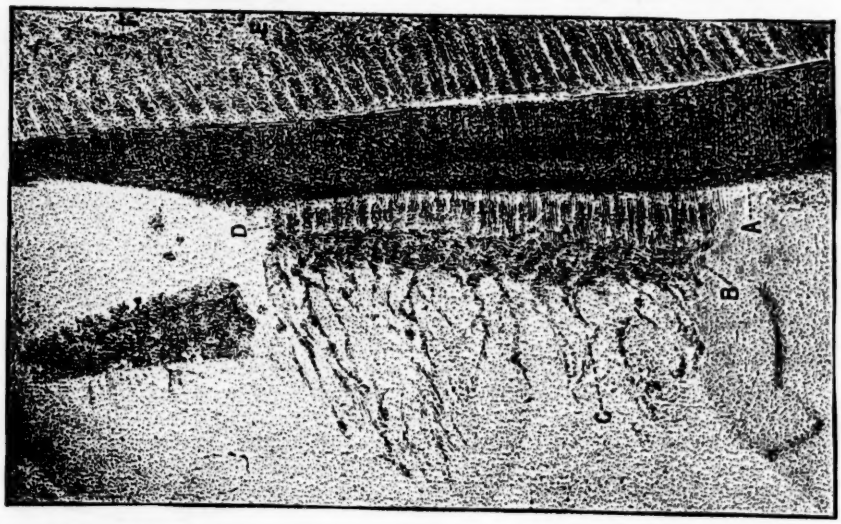

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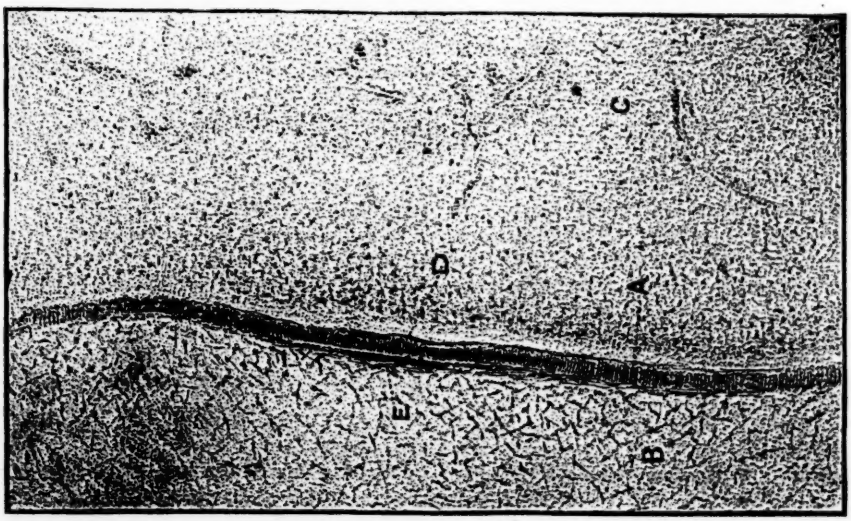
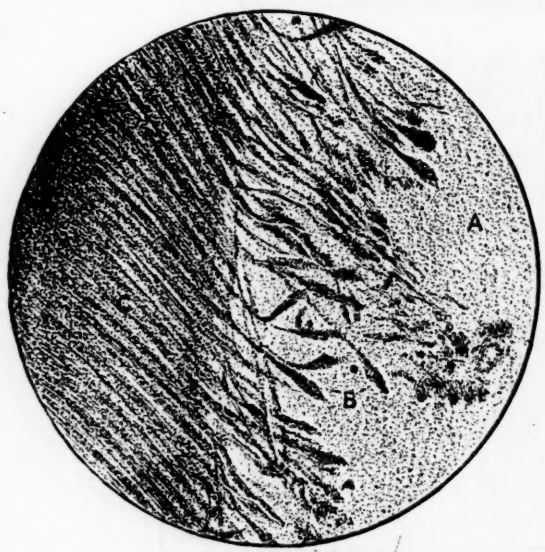


No. 4. X 300.

To ascertain, if possible, how far this is a fact, let us examine a little into some of the structural characteristics of the tissues here involved. The saculus, as before intimated, is the product of the surrounding embryonic tissue, and exhibits two well differentiated structures, the outer layer being composed of fibrous connective tissue elements of a somewhat loose texture, while the inner, or that in contact with the outer investment of the reticulum, is in structure much finer and contains a profusion of nucleated bipolar cells with long and highly refractive processes, and lying mainly parallel to the surface of the organ in the outer wall of which they seem to be interwoven and to connect upon the other side directly with the processes of the stellate cells of the reticulum. Again the processes of the cells form a like connection with those of the "stratum intermedium," and these last are seen to form true axial connections with the columnar cells of the ameloblastic layer, or, as we prefer to call it, the enamel matrix. (Plate 3, D). At the point of juncture of the cells of the stratum intermedium with those of the matrix they are joined by lateral processes. Sometimes two or more of these joinings are seen, thus forming those transverse lines which mark the upper boundary of the matrix cells, which are again joined at their lower ends where they come into contact with the dentine organ in just the same manner; and thus is formed the transverse line which marks the boundary here. And thus also is formed the supposed membrane lying between the enamel and the dentine, and which consists simply of a mesh or network composed of the lateral processes of the matrix-cells. These matrix-cells also send axial processes beyond this border line and into the dentine organ, where *nucleated* enlargements occur in them. Thus they become those columnar bodies known as "odontoblasts," to which we shall refer again further on.

We know that in these, as in the case of all the other tissues of the body, whatever elements are required for the work of building are derived from blood constituents. We know, also, that the enamel organ is *extra vascular*, the nearest approach of blood capillaries being at its outer investment (Plate 3, E) on the one side, and the dentine organ on the other. Therefore it is clear that such supply must come from the point first mentioned. The blood constituents passing through the reticulum, probably there receive such elaboration as fit them for their final place of deposit. Herein, then, consists the function of this stellate structure.

An examination of the section represented in Plate 3 shows that a change is in progress at the nuclei of the stellate cells in the immediate vicinity where a deposit of enamel is about to occur—noticeable as a *marked increase in size*. They now appear as highly refractive spherical bodies of an opalescent character. Coincidently with or a little before the first deposit of enamel, two or more courses of these bodies make their appearance in the upper part of the matrix cells (Plate 5, D), where they have been termed the nourishing cells of the enamel organ. These spherical cells, or nuclei, are not, however, developed within the enamel matrix, but are identical with those of the reticulum (C), and probably constitute the calcific elements of the enamel. They, by some means, find way within the matrix cells where, superimposed one upon another, by some unknown law, coalescence occurs, thus forming the rods or prisms of the enamel, the matrix cell walls becoming the



No. 7. X 1000.

No. 8. X 500.

cement substance between the same. In this we have a perfect explanation of the striated appearance of the enamel rods—an appearance that cannot be reasonably accounted for in any other way. It is a well known fact that by treating the enamel with alkalies, the rods may be divided into minute cubical sections just in the places where these striations occur. We submit, therefore, that each of these little sections represents an original spherical body or *nucleus* which was developed within a stellate cell of the reticulum.

Associated with the first deposit of the enamel, which occurs very nearly coincidently with that of the dentine, another very marked change takes place, consisting of an increased density in that part of the reticulum lying in contact with the matrix cells (Plate 5, B). This occurs in consequence of the upward push of the growing tooth-germ, accompanied with the increasing enamel deposit, which now so encroaches upon the reticulum that it here becomes folded or gathered back upon itself, thus forming that line known as the stratum intermedium (Plate 5, E). In Plate 4 it will be noticed that this structure is not yet developed. In consequence of this folding back of this structure it may be presumed that the nuclei lying in the deeper parts of the organ are brought into more intimate association with the matrix cells, thus favoring their migration. Plate 5 gives quite a good illustration of this. Here a portion of the matrix (D), stratum intermedium (B), and the reticulum (C), have been torn out from the enamel organ and left attached to the already formed dentine (A). As a result also of the outward push of the growing tooth, the matrix associates with what remains of the reticulum, and its outer investment becomes an attenuated layer over the apex of the tooth, and gradually assumes that form over the whole crown as the process of deposit is completed. In this we have the *mythical* "membrane of Nasmyth."

At this stage the enamelization of the tooth is nearly completed; but the function of the organ, or rather what now remains of it, does not end here, as we shall see. In Plate 6 we have represented an embryonic deciduous canine tooth with its investments at about the above stage of development. In making this section the enamel organ and the layer of last formed enamel was torn from its position, making quite a space (C) upon the labial side. Upon the palatine side the enamel organ (B) is intact, but with a disarrangement of the last formed enamel (E); yet quite a thick layer still covers the dentine, distinguishable from the latter by its higher color (F). At the base of the rudiment is seen a dark knotted line (H) composed of vessels and capillaries to cross section. Here is the growing point of the tooth. It is at this point that the crown begins and at this point that the root is finished. Immediately below this point is the base of the sac-ulus, and in contact with that the forming jaw bone (I). We note that the line (H) is joined by the lower borders of the enamel organ (B), thus completing the circuit. At this point of juncture (K) the two walls of the organ are in mutual contact. A little higher up, at (J), is the cervical line of the tooth, where the enamel is to end. Yet we see that the organ extends quite a distance below this point; in fact, nearly surrounds the dentine germ. An examination of the section reveals the fact that the nuclei in the reticulum are not developed in that structure below the points (J). Therefore this fact, taken in connection with the general conformation of the parts, clearly indicates that at this point (J) the tooth crown is to be finished, and all below that is the developing root. And it is just as evident also that in its outward growth the tooth passes directly through the remains of what once was the enamel organ, and which now becomes the matrix for the cement substance of the root and the boundary line for the dentine of the same. Inasmuch as this deposit here begins much earlier than that of the cement, it is seen that such a boundary must in some way be provided.

Here probably ends the office of the enamel organ so far as developmental function is concerned. Its life's activities having been spent in erecting to past usefulness a monument composed of the most enduring tissue of the whole body.

The main distinguishing features between the dentine and the enamel are chemical and structural. The same chemical elements enter into the composition of both, but in different proportions. This fact alone would account for a variation in structure, yet aside from this there are other important reasons for this. As we have seen, a specific organ is employed in the production of each, each working in harmony with the other and both for the consummation of the final result. In view of this one would be led to suspect a similarity in their modes of working. In studying these structures the following differences of structure are most prominent:

1. That while one is interiorly absolutely non-vascular the other is decidedly vascular.

2. In one a matrix is provided as a receptacle for the deposit; in the other no such provision is made.

3. In one a reticulated structure is a prominent feature; in the other it is hardly noticeable.

4. In one the deposit occurs in a distinct and specific form; in the other it is semi-homogeneous.

Notwithstanding, however, these well-marked differences, which we believe have often heretofore led investigation wide of the truth, we are convinced that there is very little, if any, difference in the mode of working in each. The dentine organ, as before stated, is developed in, and probably from, the embryonic elements of the submucous tissue. Some time previous to its en-

velopment by the enamel organ a rapid development of blood-vessels occurs at its base. These shoot upward into its substance, rapidly becoming a dense and arborescent system. They run mainly parallel to its long axis, with numerous fine branchings as they approach its upper boundary, where they appear to end in loops. The substance of the organ appears as a granular mass—due to its somewhat dense cellular structure. Its cells are small, variously formed, and are joined by very delicate processes, thus forming another stellate structure analogous to that of the enamel organ and probably also with the same functional characteristics, although with a widely differing environment. Some time previous to the deposit of dentine there occurs what has been supposed to be a metamorphosis of the cells along the border line in contact with the enamel matrix, which now assumes the appearance of a columnar layer at these point. These bodies, the so-called "odontoblasts," as stated above, are only nucleated enlargements of the axial processes of the matrix cells of the enamel organ, which penetrate the dentine organ along this border and are again, by fine processes, joined to the deeper cells of that structure, thus forming a continuous system. The axial connections of these columnar bodies with the matrix cells form the "dentinal sheaths" or tubuli—their axis cylinders—the dental fibres. The branchings of the "tubuli" are a result of a fusion of two or more of these bodies. We are aware that all this is very unorthodox, *but so we see it.* In this connection we should ask a careful study of Plate 7. This section was made from a growing tooth. The space (A) was occupied by the dentine pulp. The "odontoblasts," so-called (B), are left attached by their long processes to the formed dentine. As represented here, their bodies often penetrate deeply into the dentine organ. As a rule, however, they lie nearly in contact with the line of deposit. The delicate processes at the free ends of these bodies by which they connect with the structural elements of the dentine pulp are torn away.

An examination of the dentine pulp at a time when the deposit is in progress reveals the fact of a profusion of granular bodies or nuclei quite similar to those of the enamel organ, which here as there, we may infer, constitute the calcific elements of the deposit. The reticulum of the dentine pulp is by no means as easy of demonstration as that of the enamel. Very careful manipulation and fresh unmounted specimens are the conditions of success in this direction. Plate 8 represents such a structure in the pulp of an adult tooth. Since, however, this occurs where there had been a secondary deposit, an abnormal growth is to be suspected.

Upon examining the pulp of a fresh specimen of growing teeth we find, adjacent to the inner extremities of the odontoblasts, a profusion of the granular nuclei referred to above as constituting the elements of deposit. In just what manner this deposit is brought about is unknown; but the facts that it occurs in layers or laminæ, and also that these granular constituents do not lose their identity in the new formation, are easy of demonstration. By treating the dentine with alkalies we may separate it into its component parts in just the same order in which they were put together. This we have demonstrated before.

The chemical difference existing between the enamel and the dentine seems more easy of a reasonable explanation. In the dentine organ exist just those conditions requisite to cause this difference. With its dense system of blood-vessels, enclosed in unyielding walls, are furnished the conditions for excessive blood pressure required for the passage of those elements (coloids), by transudation or otherwise, which makes this difference.

Since writing the above it has, through some subsequent observations, occurred to me—almost to the point of conviction—that in its inceptive development the dentine germ is indebted wholly to the enamel organ for the stimuli requisite for such a purpose. In other words, the enamel organ is the inceptor of the dentine germ.

We know that no tooth is developed without the predevelopment of such an organ. Even in that type of teeth which have no enamel the formation of an analogous enamel organ is the first inception. This is a fact which cannot be explained by the natural or other selection, or by referring to primitive forms. Its existence, therefore, under such circumstances, would seem to imply that of a necessity it must bear some significant connection with the formation of the dentine germ.

DR. M. H. FLETCHER: The paper we have just heard and the accompaning illustrations evidently represent the expenditure of much labor and time in their preparation. It is only those who are initiated and experienced in such work, that can appreciate the hours of research, and thought and reasoning necessary in such an investigation. In the whole subject of embryology, and the various phenomena connected with it, one will not find more natural barriers and difficulties to overcome in any branch than in undertaking the study of the dental system of animals. It has claimed much attention from our most learned and talented scientists, and is still involved in great obscurity. The most excellent paper just presented, clearly indicates a love for the work which would accomplish great achievements in this direction under favorable circumstances and opportunities.

The illustrations shown in connection with the essay, represent various stages of development

from the dipping down of the mucous membrane, to the development of both enamel and dentine. As to the time of this first appearance and dipping down, the essayist does not differ from other investigators. But the "dental groove," which he speaks of as not always being present, should be called the "dental ridge." Sudduth says on this point: "Concomitant with the formation of the ridge, the proliferation of the cells of the infant layer causes a depression of the subepithelial layer lying immediately underneath. Were we to lift up this thickened epithelial layer, it would leave behind a groove in the underlying tissue. But let it be remembered, that in lifting up the ridge or rampart of epithelial cells, we have made the groove. It is never a groove *per se* but, when formed, is always an artificial product which can be made at will." The "dental groove of writers," as quoted, is therefore now obsolete.

The essayest also says that at this period, "a change is seen to occur at certain points in the cells of the epithelium consisting of an enlargement of the cells and their nuclei, these points corresponding nearly to the position of the future teeth." This enlargement of the cells and nuclei at this stage has not, to my knowledge, been noted by other writers. But some authors speak of a proliferation of the cells, their size remaining the same until the dipping down of the cord has occurred, at which time some investigators have claimed also that the cells of the malpighian layer assume a columnar form. However, the latest investigations seem to show the cells of these layers to be oval or cylindrical shaped until the cord comes into contact with the dentine papillæ, and, after this occurs, they take on the columnar form and evidently become the ameloblasts.

The stirrup shape of the enamel organ spoken of is only the form seen on making sections of the tissue. As a matter of fact, the organ is bell-shaped, forming a hood over the dentine papillæ. It is spoken of as a cap, however, in another part of the paper. At this stage the doctor shows a section in which he claims that, "at the point where the enamel organ embraces the cervical portion of the embryonic tooth, the wall of this organ is folded upon itself, thus bringing both walls into mutual contact." (See Fig. 3.) So far as I know no other author has observed this. Other investigators represent this portion as free from such conditions as described. But since this is not a matter closely concerned in any part of the development it is not of great significance.

The important point in the paper, and that which has the greatest claim for originality, is the theory of the origin of the odontoblasts. After speaking of the numerous lateral processes given off by the ameloblasts or "matrix cells," he says: "And thus is formed the supposed membrane lying between the enamel and the dentine, and which consists simply of a mesh or network composed of the lateral processes of the matrix cells. These matrix cells also send axial processes beyond this border line, and into the dentine organ where nucleated enlargements occur in them. Thus they become those columnar bodies known as odontoblasts." This certainly gives to the odontoblasts an entirely new origin, for they have up to the present been considered as a modification of the cells of the subepithelial tissue in like manner as the ameloblasts are a modification of the malipighian layer. The layer of cells at the summit of the dentine papilla, which come into nearest contact with the enamel organ, are the ones thus converted into odontoblasts.

To claim that any lateral process of a cell becomes nucleated and forms a cell of another class is certainly a perversion of the laws of morphology to some extent; for it would seem that if there are any truths established in this branch of science it is that, when nucleated cells divide, the division of the nucleus—*as a rule at least*—precedes that of the whole cell. If the doctor could show that the matrix cells generate a second nucleus which passes through this basement membrane, then it would add greatly to the rationality of his theory. The development of the cell, however, is a gradual process from a general to a special state. We can conceive how the subepithelial layer, in the process of differentiation, might develop from the epithelium, or the reverse. But the processes of amelification and dentification are, according to the essayist's own definition, quite different, and, in consequence, would require different kinds of cells—at least, sufficient difference to need the step from epithelial to subepithelial structure before these cells could be converted into organs differing as widely as the enamel organ and the dentine organ. Then, too, in this particular case, there is a great chance of error, for these processes are extremely hard to trace. Legros and Magitot say in regard to the process of the stellate cells: "It is a remarkable fact, that no line of juncture can be discovered where these cells are connected with each other, the various reagents failing to disclose the least trace of it." And this certainly holds good with regard to any processes given out by the ameloblasts at this stage. Nevertheless, this new theory does credit to its author and possibly may be true. But the chances are against it.

The fact that the enamel organ is to be universally found in the earliest stages of tooth development, even in that type of teeth which have no enamel, is not proof of the truth of this theory. For it is certain that many cells act catalytically on their environments. It is therefore not improbable that this action is the influ-

ence given out from the enamel organ, and is necessary for the incipient development of the odontoblast layer.

Reverting to the subject of the enamel organ, we may say that the description is good of the manner in which enamel is probably formed by the "subperimposing one upon another of the spherical cells, or nuclei from the 'stratum intermedium,' within the walls of the matrix cells." And also that the lateral processes of the cells and the cell walls may form the boundaries to the enamel prisms. One illustration (No. 6 B) shows the enamel organ with its stellate reticulum to be present over the particular part of the tooth in which the enamel is being deposited. But Sudduth claims there is no deposit of enamel until the reticulum disappears. The illustrations, if properly understood, would disprove Sudduth's theory on this point. That the remainder of the enamel organ form Nasmyth's membrane seem to be pretty well established. But the essayist seems to look upon the existence of this membrane as a myth.

The doctor would make this enamel organ a most versatile affair. For according to his theory it first prepares and forms the enamel, then gives the necessary cells to the dentine organ. Without it he thinks dentine could not be produced. And before it ends its existence he has it enclosing the root of the tooth, and producing the cement which is much less like enamel than dentine. This latter function of the enamel organ is certainly a new one, and it will be difficult to maintain the premises upon which the doctor's conclusion in this connection is based. Since cementum assumes so nearly the form of bone, the two processes of formation must be very much the same, and consequently produced by similar organs. Cementum is claimed—and most properly—to be a sub-periosteal product stimulated into growth by the same causes that effect the formation of subperiosteal bone. The difference between it and true bone is possibly due to the confined limits in which this tissue is necessarily deposited. Since the periodental membrane, according to Sudduth, has the special superintendence of this deposit, and is simply a continuation of the periosteum. The modification compared to bone can be accounted for easily in the manner described. But what part the enamel organ can possibly have in the production of cementum, either primarily or secondarily, is not easy to see. Moreover, the tissues necessary for the production of enamel are not at all necessary for the production of bone or cementum.

As to the source from which the cords of the permanent teeth are derived authors differ. But the latest investigators almost universally believe that the cords for the twenty anterior permanent teeth are developed from the cords of the twenty corresponding temporary teeth, instead of from the epithelium as the essayist thinks, and the cord of the first permanent molar from the epithelium, that of the second molar from the cord of the first, the second in time giving off a cord for the third and last tooth. There are specimens showing deviations from this rule, but they seem to be mere irregularities.

In closing my remarks I deem it just to say that since there is no standard in this matter, on account of the extreme difficulties surrounding such minute investigations, all that we can do is to compare the work of the essayist with that done by other late investigators. The training and labor necessary even to follow the steps of the established processes and phenomena in this work are not small. And when one has become so familiar with the microscope and the preparation of specimens that he has perfect mechanical familiarity with his work, then comes the original work which he must undertake. But just here we meet with our greatest difficulty, for we have reached the limit of our appliances and reagents. We can now only let the imagination go on and, reasoning from homologies and analogies, form theories and try to establish them. As improvements may hereafter be made in our means of investigation our theories must stand or fall as they are found to be based upon facts or fiction. And it must be remembered that these improvements are generally brought about by the discoveries and the demands of just such faithful workers as our essayist. To him therefore are due the most hearty thanks of all interested in this or in kindred work.

ARE "MEMBRANOUS CROUP" AND DIPHTHERIA IDENTICAL? YES.

Read in the Section on Diseases of Children at the Thirty-ninth Annual Meeting of the American Medical Association, Cincinnati, May, 1888.

BY I. N. LOVE, M.D.,
OF ST. LOUIS, MO.

Since the first paper of Bretonneau, about 1821, at which time our real and definite knowledge of diphtheria had its beginning, down to the present time, the question which forms the subject of this paper has not been decided.

Earnest students and careful observers have answered affirmatively and negatively. I shall not fatigue my hearers by collating the expressions of writers who have accepted one view or the other. I shall present my own opinions based upon a deliberate weighing of all the evidence presented by trustworthy and thoughtful witnesses, and personal clinical observation extending over a period of eighteen years in hospital and private practice.

During the earlier years of my professional life I was uncertain in my position; after reading the views of Virchow, Cohen, West, Flint, and others as able, I was inclined to take a position in the

ranks of dualists. Severe scrutiny of the products of the pen of Bretonneau, Traube, Barthez, Sanné, and the tribe of unicists, left me inclined to favor their position, but special clinical opportunities and additional arguments of later watchful workers and able annalists like Jacobi and Struempel have enabled me to crystalize my convictions and prompted me to take a position positive and pronounced in favor of membranous croup and diphtheria being identical.

While it may be true, as Hilton Fagge has observed, that the more our knowledge of disease advances the more our distinctions and subdivisions multiply, yet the tendency toward unnecessary multiplications and distinctions without differences should be guarded against most carefully.

Dermatologists have given us half a dozen different terms to be applied to the different forms or expressions of erysipelas when, as a matter of fact, we know that, pathologically speaking, they are all one and the same disease, dependent upon the same germ and only varying in degree, action, or manner of announcement.

So too with fevers, the same disposition was manifested, as was illustrated when for a time typhoid fever, plus malaria as a complication, permitted the new coined term, "typho-malarial fever," to take its place in our nomenclature.

Whether the membrane, as in the so-called true croup, be a fibrinous exudation, superficial and easily stripped from the surface, leaving a smooth mucous surface, only robbed of its epithelium, while that of diphtheria is more of a coagulation penetrating or poured into the mucous tissue—a necrosis, as it were, in which the discharge can be removed only with great difficulty—is not important, it being largely dependent upon the anatomical characteristics of the parts involved.

That the disease is due to a special germ or microörganism, is admitted, and the recognition of this pathological point in the treatment has made a much more favorable showing in the mortality reports.

The growth and development of seed depend much upon their individual virility, favorable soil and surroundings. The expression of every disease varies with the individual victim, and is largely affected by its environment, favorable or unfavorable. A. may have typhoid fever so mild as to lead his physician to fancy he has in hand a simple case of continued fever; an attack of scarlet fever so simple as to be almost frivolous to the unwary; an assault from the dreaded Asiatic scourge—cholera—so mild as to take the form of a gentle intestinal relaxation; an erysipelas suggesting an erythematous flush; a small-pox with pustules few and far between, and little systemic suffering, while B. in the same ward may fall a victim to typhoid, typical; scarlet fever, severe and superbly perfect; cholera, classical, and collapse prompt; erysipelas so excessive as to endanger life and occasion an amount of suffering indescribable, and a variola so virulent as to promptly kill, or leave its victim marked in visage repulsive. This being so, then why should not A. be as likely to have diphtheria in a manner so moderate and superficial as to be almost overlooked, and B. so pronounced as to be fatal before the local expression could be observed.

Diphtheria selects by preference the pharynx rather than the larynx. The tonsils are a favorite site for the infection, not alone because of their prominence, but also because, as has been demonstrated, there is frequently to be found an interruption or break in their protective epithelial covering, and also owing to the fact that they are frequently in a vulnerable condition owing to previous or present inflammatory conditions. Then again, pavexment epithelium is much more susceptible to attack than the ciliary variety, the latter being a higher grade of organization, of a more complex character, and posessed of greater ability to oppose aggression. When diphtheria attacks the larynx and trachea, which it does, fortunately, very rarely relatively, owing to the ciliary covering being less easily penetrated by the bacillus (possibly the tubercular bacillus makes a primary attack upon this point so seldom for the same reason), the free distribution of mucous glands which flow freely creating an exosmosis rather than a endosmosis, antagonizing absorption and impeding the peril of the adjacent tissues and destructive necroses of the same. At the same time the secreted mucus aids in throwing off the exudations from the surface, and the same causes are an explanation of the failures of the lymphatics of these structures to take up the infectious matter—hence laryngeal and tracheal diphtheria are largely local and unaccompanied by constitutional symptoms and would be a really mild expression of the disease were it not that for mechanical reasons life is endangered.

For similar reasons the circumscribed patches of diphtheria membrane upon the tonsils frequently covering them in their entirety, or their opposing surfaces, are unaccompanied by much constitutional disturbance; glandular involvement, etc., are mild manifestations of the disorder, and by practitioners who intemperately assert that death is the only evidence deciding in favor of a diagnosis of diphtheria, are no doubt sometimes placed in line with non-infectious diseases.

The structure of the mucous membrane, its different elements, as the epithelium, basement membrane, underlying connective tissue mixed with elastic fibres, the blood-vessels, nerves, and gland ducts, all affect materially the pathological plan of action locally and the general involvement. The free flow of secretion from the membrane lining the larynx and trachea, and the nasal cavities favors the separation and ready expulsion of organized exudates, but the difference in the ad-

jacent tissues is manifest in the varying systemic poisoning; in laryngeal and tracheal diphtheria, Bowman's membrane forms a barrier to ready absorption, while nasal diphtheria is deadly dangerous, due to the large number and size of the lymphatic ducts of the Schneiderian membrane, as well as their perfect communication with the lymphatic glands of the neck, all aiding ready entrance into the circulation.

The urine being free from albumen in the majority of cases of croup, is cited as an argument by the dualists to sustain their position, but even granting the fact, it is explainable by reason of the constitutionally mild character of the attack, if the larynx be primarily involved, and the large mortality preventing a fair comparison and complete determination of presence or absence of albumen. The frequency or infrequency of subsequent paralysis presented to prove their position as dualists can be met by the same answer as the above. A very interesting discussion of this subject occurred in the Louisville Clinical Society, January, 1888, and is reported in the *American Practitioner and News*, of February 4. An exhaustive aggregation of arguments from the dualist standpoint are presented by Dr. John A. Ouchterlony. Dr. W. Cheatham, of Louisville, by request of Dr. Ouchterlony, looked up the authorities for the other side and admirably arrayed them in the service of his friend, and he acknowledges that the resulting article comes very near making him a unicist. If there be a difference it is far from clear to his mind.

The argument that the suddenness of the attack and absence of a period of incubation separates croup from diphtheria, is properly met by the statement that examinations of the pharnx and nasal spaces are frequently insufficient, and the mild prodroma which would of necessity occur in primary laryngeal or tracheal diphtheria would probably be overlooked. I suspect that if the majority of cases of croup could tell their own story they would relate the fact that a complete examination, with full illumination of all the available mucous territory had not been made, and that intelligent and persistent interrogation might have revealed a history of slight loss of appetite and discomfort in swallowing difficult morsels for several days before.

To illustrate this point, I recall three cases of croup, so-called, within my knowledge during the past six months. One case will illustrate the three. Sent for to see a child 4 years old, said by parents to have sore throat, and feared diphtheria; not being in office some hours elapsed and call was cancelled. A few days later mother informed me that she had treated the child with medicine ordered for a neighbor's child with diphtheria, and had countermanded the order for me to call fearing her house would be placarded, and her business—the supplying of the neighbors with milk—ruined. Five days later I was summoned in the night, but being out another physician was called. Two days later I was called in consultation and found the child dying from pronounced laryngeal diphtheria. Intubation or tracheotomy was offered and rejected. The child died a few hours later and a certificate was given by the attendant giving croup as the cause of death.

The mortality records show an enormous increase of deaths from croup (?) during an epidemic of diphtheria.

One fact which is worthy of notice, and which is an additional argument in favor of the identity of the two diseases, is that the classical treatment for croup has for years been the free exhibition of the mild chloride (coupled with stimulation), with a view to its defibrinating effect. The secretory system has thus been stimulated, and the effect has been to favor the moistening and exfoliation of the exudation and antagonize the disposition to constitutional involvement.

Since the same plan of treatment has been applied to general diphtheria the tendency has been to the securement of a similar result, and the mortality reports present a more favorable showing. By the prompt recognition of the first appearance of diphtheria, and the immediate institution of imperative interference in the shape of free purging with mild chloride, local antiseptics rendering the infectious matter innocuous, and the continuance of constitutional measures which are germicides and stimulators of glandular action, first on the list being the bichloride, benzoate of soda, and large quantities of water, we can without doubt claim accomplishments that are tangible and positive.

I feel strong in the conviction that "croup" and diphtheria are one and the same disease, and that the teachings of pathological anatomy, as well as the clinical symptoms will justify no other conclusion.

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A NEW METHOD OF TREATMENT OF DISEASES OF THE URETHRA, BLADDER, UTERUS AND RECTUM—DRY MEDICATION, DRY SYRINGE.

Read in the Section on Surgery and Anatomy at the Thirty-ninth Annual Meeting of the American Medical Association, May, 1888.

BY ELMER LEE, M.D.,
OF ST. LOUIS, MO.

It is only the fact of defect, or lack of perfection, that permits of progress. Absolute perfection is not attainable. Evolution in medicine as in every other thing promises only progress. But it is a moderate forecast that asserts the certainty that diseases will be mainly conquered, life made more comfortable, and a man's life extended be-

yond the fixed limit of three-score years and ten. The industry of men everywhere enduring and endeavoring to discover means and methods of curing diseases of the race is great as it is noble and good. The probability is that, erelong, some great discoveries will flash out that will expedite progress. I bring my contribution, much or little, valuable or worthless. Perhaps it may help some other one to go a step further.

The urethra of the male is a passageway chiefly concerned in carrying urine from the bladder, having a length of some 23 cm. (8 or 9 inches) between the meatus and the bladder. The size of the urethra is 6 mm. (¼ inch) in diameter, slightly less at the outer end, slightly more at the inner end. Mucous membrane lines the channel throughout, which is continuous with the inner coating of the bladder for urine and the kidney. The diseases of the urethra are similar to the diseases of mucous membranes elsewhere in the body. Inflammation of the urethra may be produced by one of the following causes: 1. Repeated mechanical or chemical irritation; 2. Catching cold; 3. Toxic and miasmatic infection. There is an endless number of known and unknown septic poisons of which many, if not all, have a specific inflammatory action.

My paper is especially concerned at this time with a method of treatment of mucous membranes of canals and cavities accessible to surgical aid. The male urethra is the seat of frequent inflammatory disease, and infection is the common cause, and the diplococci or gonococci of gonorrhœa the most common cause. The treatment of the urethra for gonorrhœa and other inflammations concerns a vast number of persons.

The methods of treatment are numerous. It is to-day, in the light of science, not a question so much as to what remedy or drug is required to destroy the cocci and other bacteria, and which drug is a good disinfectant, and which an anæsthetic, but the prime question is, how to use the drugs with effect and without harm to the unfortunate patient.

I think it is a matter as to how, more than as to what, to use. My method of treatment of diseases of the urethra is as follows:

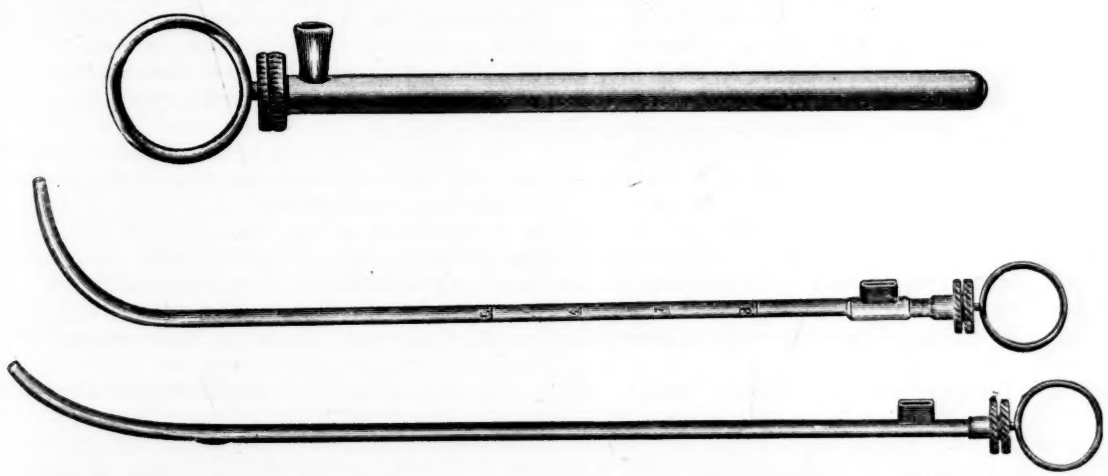
1. The employment of alkaloids, salts of alkaloids, glucosides and pure chemicals, in the dry state.

2. The active principles are in the form of granules or small masses mixed with pure cane sugar, and as soluble as the nature of the active agent will admit.

3. The placing of one or more granules at a dose, whether for purposes antiseptic, disinfectant, anæsthetic, astringent, and so on, in the precise region of the disease, and leaving the medication just where it is deposited. As it melts and dissolves and mixes with the secretions the tract or region of tissue will be bathed as the substance finds its way over their surface.

4. The use of instruments adapted for the purpose of carrying the granules in.

The matter of the selection of the medication is in the hands of the surgeon to use that which he may. The form of the remedy is convenient, the dose is accurate, and the method of introducing it into the channel of the urethra, the bladder, the cavity and neck of the uterus and the rectum is clean, economical, elegant, effective and simple. The warmed and oiled instrument is carried down into the urethra until the end defines the inflamed patch, and this it does with accuracy, when the granules are deposited in the urethra and the instrument withdrawn. The size of the granule is such that only the slightest, if any, irritation is caused. There is absolutely no distress as is the case with bougies of gelatine and masses of medicated gelatine and cocoa butter, and so on. The strength of the substance of the granule will determine as to how many to use for one



dose. A single granule of corrosive sublimate of 1 millig. ($\frac{1}{64}$ grain) is equivalent to a teaspoonful of a sublimate solution of 1:2000. Of granules of iodoform, iodol, hydro-naphthol, thallin sulphate and cocaine hydro-chlorate, as many may be deposited as may be needed in the judgment of the surgeon. The solubility of the granules *in situ* is considerably increased on account of the acid secretion of the mucous coat.

In the urethra the salts of alkaloids dissolve in three to six minutes, the alkaloids take a little longer, and the pure chemicals in three to six minutes. The medication is in a form so small and even when mixed and dissolved in the acid secretion, is inconsiderable in volume, a condition which favors thorough medication, in that the whole substance is retained on the mucous surfaces and not expelled as it would be if it were of a solution injected, or of such bulk as would cause distress to the urethra and expulsive contraction in the uterus.

A few days before I left home, my neighbor, Dr. Steele, used the uterine douche and a solution of boric acid in the case of a patient, the object of which was to disinfect the cavity of the uterus of some petrifying *debris*. Some particle of substance was forced into a venous sinus thereby, partial collapse ensuing quickly, which was with extreme effort overcome. Dr. Steele said to me, "Would the *granules* of boric acid not have been equally efficient? I think they would. And would there have been the same risk to life? I think not."

The granules are made of the active principles and pure chemicals and contain absolutely no irritating material. The size of the granule is not to be more than 3 mm. ($\frac{1}{8}$ inch) in diameter, for obvious reasons, and will contain of substance 1 centigram ($\frac{1}{6}$ grain), 1 milligram ($\frac{1}{64}$ grain), and of a few substances ½ milligram ($\frac{1}{128}$ of a grain). The alkametric granules of Frederick Stearns & Co., of Detroit, made chiefly of Merck's pure crystalline salts, are suitable for my method. Also, the dosimetric granules of Chanteaud, in such high favor in France. The granules of the Metric Granule Co., of Chicago, are especially suitable by reason of the uniformity of size of the granule.

I have devised three instruments, which I will presently exhibit to you, as well as my method upon a patient. The short, straight instrument, consisting essentially of a plunger and barrel with its adaptation to a new application, is, through the kind suggestion of my friend, Dr. C. H. Hughes, christened a "dry syringe." It is applicable to the urethra and rectum. The second instrument is 25 cm. (10 inches) long, and consists of a tube and a flexible piston rod, with the proper curve to pass into the deep urethra and bladder. The third instrument is a modification of the dry syringe for the urethra, and is applicable to the uterus and cervix. The dry syringe for the urethra is for the patient to use under instruction from the surgeon and may be prescribed or supplied to his patient by the physician. The dry syringe for the bladder and uterus are, of course, only safe in the hands of the skilful surgeon.

The time since receiving my dry syringe from the maker was too short to do more than demonstrate the mechanical features of this method. At the next meeting I will bring the results before you of a year of clinical experience with the dry syringe and granules of pure substances in the treatment of diseases of mucous membranes of the pelvic organs.

316 North Ewing Avenue.

THE CARBON COMPOUNDS.

THEIR TRUE PLACE IN THE TREATMENT OF FEVERS—OR THE PARTICULAR TYPES OF FEVER IN WHICH THEY ARE INDICATED—WITH SPECIAL REFERENCE TO THE EMPLOYMENT OF AMMONIUM SALICYLATE IN THE TREATMENT OF TYPHOID FEVER.

Read before the Virginia State Medical Society, at a Meeting held in Norfolk, Va., October 24, 1888.

BY S. K. JACKSON, M.D.,
OF NORFOLK, VA.

The older members of this Society may remember that about fifteen years ago (1873) I had the honor of presenting a paper entitled "Some Points in the Pathology of Typhoid Fever which Furnish Indications of Treatment," in which I suggested a plan of treating that disease differing greatly from those usually pursued. I think it due to the Society that it be kept informed of the history of a suggestion in which some interest may be felt, from its having been first made on this floor, and which has since attracted considerable attention.

My object now is, not only to fulfil this obligation, but also to comment upon a modification which has been proposed. And this will lead us to a discussion of the true place of the carbon compounds in the treatment of fevers, and to show that their selection is of much consequence, and becomes almost a matter of mathematical calculation, while their effects can be ascertained with almost mathematical certainty.

The favorable reception of the paper alluded to induced the editor of the *Virginia Medical Monthly* to solicit another for his journal, with fuller details of treatment and reports of cases in which it had been used. These papers evoked a large correspondence; probably necessitated by some unfortunate typographical errors. After correcting these as far as possible by letter, and after the lapse of some ten years, I felt that the results of maturer study and more extended experience should be laid before the profession; and this desire culminated in a paper on the "Ammonia Treatment of Typhoid Fever," read before the

Section of Practice of Medicine, at the meeting of the American Medical Association in Washington, in 1884. This paper was published in THE JOURNAL in August of that year—vol. iii, p. 183.

At the solicitation of the editor of the *Physician's Magazine*, this paper was supplemented by another giving reports of cases showing the effect of the sedative salts of ammonia in reducing temperature, and which was published in vol. i, p. 32, of that journal. Numerous letters, some of inquiry, some reporting experiences, some expressing grateful acknowledgments, poured in from all quarters. THE JOURNAL in an editorial called attention to the original paper by a kind and favorable comment, and then again on October 10, 1885, gave an abstract and recommended a re-reading of it. My object in making these allusions is merely to show the amount of interest awakened in the subject.

These notices were followed by an exceedingly interesting paper by Dr. J. R. Barnett, of Wisconsin, relating some cases in which he had employed, with the most gratifying results, the treatment as proposed by me, and then giving an account of his experience with a modification of it suggested by himself, viz.: the substitution of ammonium salicylate for those salts of that base which had been proposed and used by me (THE JOURNAL, December 11, 1886). This drew from the pen of Dr. J. D. Sullivan, of Brooklyn, N. Y., a paper read before the Fifth District Branch of the New York State Medical Society in May, 1887, and of which a reprint was subsequently published in pamphlet form, giving a report of cases in which he had used Dr. Barnett's modification. This elicited from THE JOURNAL (August 6, 1887, p. 179) another complimentary notice of my paper, with the declaration that it would repay a re-reading.

Dr. Sullivan's paper was followed by a paper by Dr. D. M. Wick, of New Hartford, Iowa, published in THE JOURNAL, vol. x, p. 164, February, 1888, giving some interesting cases in which he had employed the ammonium salicylate.

The next paper on the subject which has come to my notice, is in course of preparation by Dr. E. S. Ellis, of Manistee, Mich.

The last two numbers of THE JOURNAL (September 29 and October 6) contain another elaborate paper by Dr. Barnett, entitled, "The Antipyretic and Abortive Treatment of Typhoid and Remittent Fevers," in which he condenses all that has been written on the use of ammonium salicylate in fevers.

While these several papers manifest an appreciation of the nitrogen compounds in the treatment of those fevers akin to typhoid, they fail to make distinction between this class of fevers and those of malarial origin, and thus ignore the fundamental principle upon which this treatment is based; and the object of this paper is to show the necessity of this differentiation; for, while the carbon compounds are essential in the one class, they are of no account in the other—that, though they are required in malarial or bilious fever, typhoid fever and fevers of its class as imperatively demand the nitrogen compounds; and as the ammonium salicylate combines the two classes of remedies, it is more properly adapted to that form of fever which seems to be a compound of both classes.

As to the efficacy of this agent in the septicæmic fevers, it is not my purpose now to discuss.

As an analysis of the cases reported by the several gentlemen will confirm the positions for which we are contending, let us glance hurriedly at them.

The cases in which Dr. Barnett reports having tried the "ammonia treatment," and with marked success, were probably true typhoid fever, while the first in which he used the salicylate was, as he says, remittent fever. Several similar cases followed and, being *similar*, must have been of malarial origin. The next was a case of puerperal septicæmia; the next was of the same nature. Then followed some dozen cases, a majority following early abortions, in nearly all of which cellulitis was present. The next was gangrene of the lungs; next the septicæmic state of the last stage of tuberculosis—pneumonitis complicating other affections—next cerebral meningitis. Some twenty-five or more cases are alluded to, but not reported, of typhoid or allied fevers; but, as Dr. Barnett recognizes a close clinical relationship between these and remittent fever, we cannot know to which class they belong.

Dr. Sullivan's cases were mainly of erysipelas, septic cellulitis, puerperal septicæmia and the septic fever of tuberculosis. He had the opportunity of employing the salicylate in only one case of typhoid fever.

Dr. Barnett alludes to a paper by Dr. S. A. Fliesburg, of Hudson, read before the Inter-State Medical Association in July last, but as I am unable to procure it, have no means of knowing the class of cases reported.

Dr. Wick's cases are easily analyzed. He reports five, not one of which can be considered to be typhoid fever. The first he calls a remittent; the second remittent; the third measles; fourth atypical typhoid. The fifth was probably an intermittent.

Thus we obtain from these cases very little or no evidence of the value of the ammonium salicylate in uncomplicated typhoid fever.

While it has been a gratification to receive from practitioners in almost every State of the Union evidences of their interest in, as well as favorable reports of their trials of, the ammonia treatment of typhoid fever, as proposed by me and practiced with marked success for the past forty years, it has also been a matter of some surprise that so revolutionary a treatment, and one for which so

much is claimed, has not attracted the attention of our schools. Might it not be considered a duty they owe to the profession, either to approve suggestions worthy of trial, or to condemn such as are unsound in principle and likely to be harmful?

It may be that the learned professors of our colleges regarded, as Dr. Barnett was inclined to regard, the theories upon which this treatment was based as mere "vagaries;" but Dr. B. declares, as I had already shown, that they had borne "substantial fruit;" and it is reasonable to infer that a tree that could bear "substantial fruit" was not a myth, but a substantial tree. "A tree is known by its fruits." "Men do not gather grapes of thorns or figs of thistles."

Several of the journals have been furnishing us, lately, with details of the treatment of typhoid fever which is pursued by the most eminent and popular practitioners in the great medical centres of the country; but as these plans of treatment differ widely from that proposed by me, it is to be supposed that they are based on entirely different pathological views from those which suggested "the ammonia treatment." The correctness of these views can now only be inferred, until more accurate scientific investigation can be had, from a comparison of results. I have already reported my rate of mortality (which is less than 1 per cent.), and it would be interesting to compare it with that of the other modes of treatment. Dr. Barnett estimates the mortality by salicylate of ammonia treatment as 3 per cent. Of the others we have no reports.

As stated above, Dr. Barnett has proposed the employment of the ammonium salicylate as a modification of my treatment, and the object of this paper is to show to what class of fevers this valuable remedial agent is applicable, and why neither it nor any of its congeners are demanded, nor are particularly beneficial, in pure uncomplicated enteric fever.

In determining the treatment of a case of fever it is important to ascertain what particular form of fever we have to combat. We are not merely attempting to subdue the elevated temperature common to all fevers, but must aim our attack at the agency which has produced the pyrexia. If the former were our aim, the mere application of cold might effect our purpose; but if the latter, we must endeavor to ascertain the pathogenic cause, which, if as yet beyond our reach, can be studied as to its behavior, as to the conditions most favorable to its propagation, as to the agencies capable of destroying or crippling it, and as to the particular lesions caused by it. So far from recognizing any relationship, either "pathological or clinical," between malarial (that is remittent) fever and typhoid fever, I regard these two forms of fever as entirely and essentially distinct and possibly antagonistic—that they do not prevail in the same district at the same time; that they originate from different causes and require different treatment. I think I am right in declaring these to be the views generally entertained by practitioners in malarious districts.

I have known some physicians to distinguish between them by testing the ability of quinine to control them or not, (a clumsy and unphilosophical mode of diagnosis) which, while distinguishing between the diseases, demonstrates at the same time their diverse character, as well as the fact that the most efficient means we possess for controlling malarial or remittent fever has no power in enteric fever.

While, in making diagnosis between these two classes of fever, I have not been puzzled by the "insensible shading" of which Dr. Barnett speaks, I admit the existence of a form of fever which really seems to partake of the characters of both. As every one who has read my previous papers on this subject will remember, that I was inclined to regard what is called "typho-malarial fever" as a disease *sui generis*, and due to a cause differing from that which produces true typhoid fever, and that I had considered these two forms of fever antagonistic, as stated above. But subsequent observation has inclined me to modify this opinion, having recently seen cases in which the malarial symptoms disappeared and left the typhoid symptoms uncomplicated, and *vice versa*. In a case just recovered the typhoid symptoms disappeared two weeks before convalescence was established. These hybrid cases, as I will show presently, are those in which Dr. Barnett's modification of my treatment is indicated. In these cases I have lately tried it with the most satisfactory results; but in pure uncomplicated enteric fever I have not seen, nor, for reasons which I have already given, did I expect to see, effects comparable to those of the other ammonium salts. These reasons are probably what Dr. Barnett considers "vagaries," but it is gratifying to know that the accurate researches of modern pharmacology are daily adding new facts confirmatory of them.

I do not know that any one contends, at this day, that the different forms of fever originate from the same cause—that the organism causing malarial fever was identical with that causing typhoid or yellow fever. This would be as preposterous as to suppose that small-pox and scarlet fever were caused by the same micro-organism.

In the account of his researches into the hæmatozoön accompanying, if not causing, malarial fever, Osler remarks: "It is very evident that we are dealing here with structures unlike any others which have been described in human [illegible], and with bodies which have no relation what[illegible] to the spirillæ, micrococci, and bacteria of certain acute diseases." And Laveran declares that "these hæmatozoa have never been met with elsewhere than in the blood of patients suffering from malaria." These hæmatozoa are so distinct

from the typhoid bacillus in their morphology, in their habitats, in their life-history, in the food they require, in their excretions, in the lesions or effects produced in the human system, no one could suspect a relationship between them, or could suppose that the same toxic agents would be equally deleterious to both. Experience and observation have proved that they are not, and have demonstrated that quinine, the great antidote to the malarial poison, has no effect upon that of typhoid fever. This agent has been proved to be actually destructive of the micro-organism of malarial fever. Laveran declares that "he has never seen the peculiar hæmatozoön of malarial fever *after quinine has been taken*, and in order to find it, it must be looked for before any cinchona preparation has been administered." Has this ever been affirmed of the typhoid bacillus?

Whether the theory by which I attempted to explain the efficacy of certain means in these fevers be accepted or not, it is certain that the carbon compounds have more in conquering malarial, and that the nitrogen compounds are most efficacious in typhoid fever. To the former class belongs salicylic acid, and to the latter ammonium, and therefore the ammonium salicylate may be said to belong to both classes. Few practitioners would be willing to substitute salicylic acid for quinine in malarial fever, nor could I be persuaded to dispense with the doubly nitrogenous salt (nitrate of ammonia) for one with less nitrogen in typhoid fever.

The study of these carbon compounds is one of the most interesting engaging the attention of the profession; and by it some curious facts are being daily developed, which are strongly confirmatory of the theory I have advanced.

Brunton says of these carbon compounds "that their chemical nature depends on the arrangement of their constituent atoms, but their physical character depends on the number of atoms." He might also have said that their antipyretic, antiperiodic and antiseptic power increases with the number of *carbon* atoms which they contain. This rule, as far as their antiseptic power is concerned, is undoubtedly true, especially with regard to those organisms whose peculiar function it is to produce fermentative processes by which there is an exhalation of carbonic acid (CO_2). To see how far this assertion is justified let us glance at the series.

The lowest in the scale; that is, those with the smallest number of carbon atoms, are the alcohols, which are diffusible stimulants, evanescent in their effects. Methyl alcohol, the lowest (CH_4O), produces excitement quickly and runs through its stages rapidly. With ethyl alcohol (C_2H_6O) the stages succeed each other slower, but in regular order, and convulsive phenomena appear. Whereas in œnanthylic alcohol ($C_7H_{16}O$) and caprylic alcohol "the stages are irregular and confused and convulsions sooner occur."

While I have spoken of the lower carbon compounds (the alcohols) as *stimulants*, I do not wish it thought that I ignore the modern idea that they are such by destroying the normal balance between the inhibitory and excitor nerves, by paralyzing the former without directly stimulating the latter. This paralyzing effect seems to be in direct proportion to the number of carbon atoms contained, and the law for which I am contending confirms the modern theory alluded to; for, as we ascend the scale still further, the paralyzing or inhibitory effect is still more pronounced.

The next higher alcohol is amylic alcohol ($C_{10}H_{12}O_2$), from which the powerful agent nitrite of amyl is prepared, which "produces *paralysis* of the nerves" (Brunton).

The ethers and aldehyde each contain but two atoms of carbon, hence their action is rapid and not persistent. But paraldehyde containing $C_6H_{12}O_3$ "is a pure narcotic causing sleep, like chloral."

Chloral and chloroform, the one having two and the other but one atom of carbon, seem to be exceptions to this rule, but this small proportion of carbon may account for the evanescent character of their anæsthetic and analgesic effect, and also for their want of antipyretic and antiseptic power.

As we ascend to the aromatic series of the carbon compounds this fact becomes still more prominent. We then reach carbolic acid (phenol alcohol, C_6H_5HO), creasote, resorcin, and hydroquinone, all of acknowledged antiseptic power.

The next step leads us to salicylic acid ($HC_7H_5O_3$), which though admitted to be germicidal, Brunton says of it, "It is much less useful in typhoid fever, and although it has some antiperiodic action, it is not such a powerful remedy in malarial affections as quinine."

The next in order is chinoline (C_9H_7N), containing two more atoms of carbon. It is said to be a powerful antiseptic and antipyretic (Brunton). "It has been used in typhoid fever, etc., apparently with benefit," probably because of the nitrogen it contains.

Naphthol ($C_{10}H_7OH$) contains one more atom of carbon than the last and is said to have a therapeutic action on the skin, like tar, which is used with efficacy in several parasitic skin diseases. When absorbed it causes loss of consciousness and convulsions.

Turpentine, a generally recognized antiseptic, contains the same proportion of carbon ($C_{10}H_{16}$).

The next higher in the series is one of the newly-proposed antipyretics, whose powers in this respect are unquestioned, viz., kairin, containing C_{11}, or one more atom of carbon. It is a salt of ethyl (C_2H_5)—chinoline (C_9H_7N)$=C_{11}H_{12}N$. We next reach the still more powerful antipyretic, "antipyrin," with its fourteen atoms of carbon and two of nitrogen. This climax is surmounted

by the most powerful destructive agent we possess to the malarial microbe, viz., quinine, which contains twenty atoms of carbon ($C_{20}H_{24}N_2O\ 3H_2O$).

A glance at this list will vindicate the assertion made at the outset, viz.: That the more atoms of carbon which an agent contains the more powerful is its germicidal and, therefore, its antipyretic effect. But this is only true, as we have shown, as far as the malarial poison and its congeners are concerned. For an agent capable of destroying the typhoid fever poison we must look to another class containing more or less nitrogen, which we have heretofore shown is the natural, or nature's antidote to that poison. The reasons for so considering it we will not repeat, but they are so cogent, and the success of the nitrogen compounds has been so decisive as to induce me to abandon all other means in the treatment of typhoid fever. No other reported plan of treatment compares with it in results, and it is not venturing too much to prophesy that it is destined to become the established treatment of this disease.

I hope I have succeeded in showing,

1. The necessity of a diagnosis between malarial and typhoid fevers, which is important on account of their different characters and their requiring different treatment.

2. That it is only the carbon compounds that are particularly efficacious in malarial fevers; as I have heretofore shown that the nitrogen compounds are demanded and are eminently efficacious in enteric fever.

3. That Dr. Barrett's suggestion of the use of salicylate of ammonium is particularly valuable in that hybrid form of fever to which Dr. Woodward gave the name of "typho-malarial," and not in pure typhoid fever.

4. That the carbon compounds generally are only indicated and could only be expected to be useful in those forms of fever which are due to a fermentative process caused by organisms exhaling carbonic acid gas, since they are chiefly destructive of this class of organisms and have little or no toxic effect upon those causing fevers which are accompanied by processes analogous to putrefaction in which ammonia or sulphuretted hydrogen are evolved, *e. g.*, typhus, typhoid, and the septicæmic fevers.

And finally, that these theoretical views exactly correspond with and are confirmed by clinical experience, as no agents have been found so efficacious in malarial fever as the carbon compounds; none more potent in typhoid fever than the ammonium salts; and probably none more valuable in septic fevers than the salts of sulphurous acid. And further, that this clinical experience justifies a reliance upon the use of these excretory products, viz.: carbonic acid, ammonia and sulphuretted hydrogen as agents for the destruction of the several organisms producing them, as being Nature's mode of inhibiting their life-processes, in accordance with the law "that no organism can live in its own excreta," that is, in the product of its life-processes.

MEDICAL PROGRESS.

BINIODIDE OF MERCURY SPRAY IN TUBERCULOSIS.—DRS. MIQUEL and A. RUEFF have published a work on the treatment of pulmonary tuberculosis by sprays with the biniodide of mercury. The experiments of Dr. Miquel having shown him that the biniodide of mercury is microbicide in solutions of 1 in $\frac{1}{100000}$, he was induced to try it against pulmonary phthisis, and this he has done in conjunction with Dr. Rueff. They have established spray apparatus at the Rothschild Hospital, and have submitted phthisical patients to the vapors of the biniodide of mercury, or rather the iodo-hydrargyrate of potassium. The following is the formula of the solution employed for the sprays: biniodide of mercury one part, iodide of potassium one part, distilled water one thousand parts, all by weight. But whether medicamentous liquids in the form of spray penetrate into the trachea, the bronchial tubes, and their ramifications is a question that has not yet been satisfactorily solved. Denied by some, this penetration is admitted by others, and appears to have been demonstrated by the experiments of Drs. Miquel and Rueff. It may, however, be observed, that of twenty-seven patients submitted to this treatment nineteen were improved, and eight remained stationary; the improvements were therefore at the rate of 70 per cent. In these cases attenuation of the pulmonary lesions was obtained, and particularly diminution of the expectoration and increase in the weight of the patients. In two cases even the disappearance of bacilli was established.—*Lancet*, Nov. 3, 1888.

TINCTURE OF PHOSPHORUS IN INFANTILE ASPHYXIA.—DR. FARICI has obtained excellent results in the treatment of asphyxia, occurring in the course of broncho-pneumonia in children. He advises the use of the following formula:

℞ Cinnamon water . . . . . 100 grams.
Ethereal tincture of phosphorus 8 drops.
Syrup of ether . . . . . 20 grams. ℥.

The dose is a teaspoonful every hour.—*Lyon Médical*, September, 1888.

ENEMA FOR CONSTIPATION WITH HÆMORRHOIDS.—

℞ Glycerine . . . . . 60 parts.
Soap . . . . . 10 "
Fluid ext. rhubarb . . . . . 40 "
Essence of chamomile . . . . . 10 drops ℥.

Use as an enema three times daily.—*Revue de Thérapeutique*, October, 1888.

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SATURDAY, NOVEMBER 24, 1888.

EDITORIAL CHANGE.

Having accomplished all that he had hoped or expected to accomplish when he accepted the editorship of THE JOURNAL OF THE AMERICAN MEDICAL ASSOCIATION in 1883, and feeling the necessity for diminishing his responsibilities and constant work, the Editor of THE JOURNAL tendered his resignation to the Board of Trustees in the latter part of June last, the same to take effect on the 31st day of December, 1888. At a special meeting of the Board of Trustees held in Chicago, Nov. 9 and 10, 1888, the resignation was accepted, and JOHN B. HAMILTON, M.D., Supervising Surgeon-General of the Marine Hospital Service, and late Secretary-General of the Ninth International Medical Congress, was chosen unanimously to fill the place. He will enter upon the discharge of his editorial duties January 1, 1889. We trust he will have a long and prosperous editorial career.

AN IMPORTANT QUESTION.

"What will be the final effect upon the American Medical Association of the various associations of so-called *specialists*—the American Surgical Association, the Association of American Physicians, the Gynecological Association and all the others? It will be a sad day for the profession of America when the time-honored A. M. A. ceases to be *the* medical institution of the country."—*New Orleans Med. and Surg. Journal*, November, 1888.

The question thus stated by our valued contemporary has been asked in a more private way by many of the more thoughtful members of the profession in all parts of the country. That many sincere friends of the American Medical Association, the true representative organization of the medical profession of this country, have looked upon the steadily increasing number of American associations of specialists with anxiety, lest they prove disintegrating and more or less antagonistic to any general and harmonious organization of the whole profession, is well known. And that some of those who have not only taken part in one or more of these specialist organizations, but have actively promoted the combination of all these in the so-called Congress of American Physicians and Surgeons, have been actuated by the desire that the latter should ultimately displace and supersede the American Medical Association, we have adequate evidence. There is now, and has ever been, in the ranks of the medical profession as in the ranks of all other educated classes, a limited number whose mental characteristics lead them to exclusiveness, or a decided disinclination to meet with the mass of the profession on a common platform. This comparatively small number, chiefly resident in the large cities, have never been satisfied with the liberal organization of the American Medical Association. They were represented in the Convention for organizing the National Association in 1846–7, and earnestly advocated a plan of organization that, while it would admit all regular practitioners of medicine to *nominal* membership, would restrict the whole business and management of the Association to a Council of limited number capable of self-perpetuation, instead of the representative plan based upon the State and local societies as reported by the committee and adopted by a very large majority. At a subsequent period, when the rapid development of specialties had commenced, it was this same underlying mental trait that prompted a few of those, in each, to meet by mutual agreement and organize *themselves* into an American Association under by-laws limiting the number to 50, 75 or 100 as the case might be, and making it impossible for any others to gain membership un-

less they were proposed and elected by nearly a unanimous vote of the existing members.

It is on substantially this narrow basis that the numerous American associations of specialists are organized, and every young specialist entering the profession, unless he has some influential friends among the members to propose him, and not a sufficient number of jealous rivals to *blackball* him, he must wait until he has actually achieved sufficient reputation to make his membership necessary to sustain the credit and influence of the association in order to gain admittance. In the meantime every important specialty is represented in the *Sections* of the American Medical Association, into which he can enter without the risk of being blackballed by rivals already in, and where he can mingle with all classes of the profession and have whatever communications or work of merit he presents spread before the profession generally through the columns of THE JOURNAL, instead of being hidden in an annual volume of transactions read by few except the members of the special organization before which they were read. The result will be that three-fourths of the young and ambitious specialists will continue to cultivate the acquaintance of the general practitioners by participating in the State societies and through them go to the American Medical Association and its Sections, and thus rapidly develop a National reputation in less time than they would have been kept in waiting to fill some vacancy in the membership of an exclusive specialist organization. Indeed, this result has already been demonstrated. During the last two or three years more work, and that of equally good quality, has been done in the several Sections of the American Medical Association than has been done in the National Associations of specialists.

And so obvious are the advantages of a wider publication of their papers, and their more direct personal contact with the general practitioners, upon whom the specialists must depend for much of their patronage, that very many of the ablest specialists who are active workers in the special societies, are equally active and influential in the Sections of the American Medical Association. Instead of the circle of American associations of specialists, accommodating in the aggregate less than 500 of the 50,000 or 60,000 members of the regular medical profession of the United States, having any serious tendency to supersede the American Medical Association, it will only serve as a convenient and really useful place in which the small but respectable class of exclusionists can work and dine without personal contact with the sunburned and weatherbeaten general practitioners of the healing art.

THE HYPNOTIC ACTION OF SULFONAL.

Several articles have recently appeared on the action of this new drug, one of which, an inaugural dissertation by MATHES, is a very carefully prepared paper. He has administered sulfonal to tuberculous subjects, cardiac patients, patients with meningitis, acute alcoholism, anæmia, etc. In short, the drug seems to have been tried in all cases, without distinction, Mathes having previously found that it was harmless in certain quantities. His experiments showed that cardiac patients bore the drug as well as phthisical patients. The results of his experiments were that sulfonal had a complete hypnotic action 72 times in 100 cases, incomplete hypnotic action 9 times, and no effect 18 times; in 19 cases accessory manifestations were produced. In most cases it acted better the second night than the first—a fact that makes it much superior to morphine and preparations of opium. The accessory manifestations mentioned consisted in ringing in the ears, slight cephalalgia, giddiness, general fatigue, and in a few cases vomiting. There were, however, no cardiac nor respiratory troubles, and no modification of the appetite or digestion.

The conclusions drawn by Mathes are as follows: 1. Sulfonal is a useful hypnotic agent, though it is not always efficacious. 2. It has the advantage over other agents of having no odor, no taste, and no influence on the essential organs of life. 3. It caused disagreeable effects in only a very small number of cases; and the worst of them was usually insignificant. 4. The dose is variable, and depends on individual susceptibility Generally a gram is sufficient to cause sleep without accessory manifestations. When these are produced the dose should be diminished. On account of its slow action it should be given at least an hour before the time for sleep. 5. When the insomnia is due to an irritating cough or to pains not clearly neuralgic, the use of sulfonal is contra-indicated. In most true neuralgias, on the contrary, it may be used with benefit.

The hypnotic value of sulfonal has been investigated also by Dr. G. Algeri, in the Asylum for the Criminal Insane at Ambrogiana, his results being published in a recent number of *La Riforma Medica*. Algeri gives records of fifteen cases of mental disease, mania, melancholia, dementia, alcoholism, etc., in which the use of sulfonal gave a calm and regular sleep. The quantity used was from 1.5 to 4 grams. The sleep produced was not accompanied by any alteration of the circulation or of respiration, and was on the average of five hours' duration. No case of disturbance of the stomach was seen, even when the maximum dose was given.

Contrary to the opinion of Algeri, Mathes agrees with Salgo in regard to the slight sedative action of sulfonal in mania and delirium. Garnier, in a recent article (*Progrès Médical*, October 13), thinks there is much promise for the use of the drug in the therapy of the insane, an opinion shared by Rabbas. Expert opinion, therefore, seems to vary but little in regard to the future field of usefulness of sulfonal. Cramer, Fränkel, Kast, Ostreicher, Rosenbach, Rosin, Schmey and Schwalbe confirm the results obtained by Mathes, and speak from personal experience of the remarkable efficacy of sulfonal in producing a sleep absolutely comparable with normal sleep. After a dose of from 2 to 3 grams this sleep lasts 5 or 6 hours without the least modification of the pulse or respiration, as mentioned by Algeri. All unite in declaring it superior to chloral, paraldehyde, and all other drugs.

DIAGNOSTIC BACTERIOLOGY.

WEICHSELBAUM has recently shown, in two cases at the Rudolphus Hospital in Vienna, the great diagnostic importance of bacteriology. The first case was that of a workman who had suffered from articular rheumatism for fifteen days. The tibio-tarsal articulation was swollen, and the temperature of the patient was 40.3° C. After the administration of salicylate of soda the temperature fell, and the pains diminished, but soon took on their former intensity. After five days the left knee-joint became affected, and in a few days a small pustule appeared. The spleen was enlarged, and the inguinal glands engorged. Weichselbaum examined some blood taken from the finger, and found the bacilli of glanders. A few days after this it was learned that three horses of the employer of the patient were dead of glanders. The patient died twenty-two days after entering the hospital. Weichselbaum found glanders-granulations in the skin, subcutaneous and intramuscular tissue, and in the lungs. It was discovered that the patient had not come in contact with the diseased horses, but he had used the blanket of one of them. It is probable, therefore, that infection took place through the respiratory passages.

The second case was somewhat different. A coachman came to the hospital with several ulcerated nodules on his face, neck, and extremities, muco-purulent discharge from the nose, dyspnœa, tumefied spleen, etc. These symptoms taken in connection with the man's occupation, lead to a suspicion of glanders. But when a bacteriological examination was made of the pus of the nodules, no bacilli were found; enormous quantities of the streptococcus pyogenes and streptococcus pyogenes aureus were found, however. The patient died in four days, and at the autopsy Weichselbaum found, besides the furuncles, œdema of the glottis and lungs, and a parenchymatous nephritis in the atrophic stage. The principal disease, therefore, was nephritis, and the furuncles and the nasal affection were secondary infections.

Weichselbaum has also seen two cases in which the anatomical diagnosis at the autopsy, could be made with certainty only by the aid of bacteriology. These were supposed cases of charbon. The patients were brought to the hospital in an unconscious condition, with paresis of the extremities, dilatation of the pupils, etc., and died in about two hours. At the autopsies Weichselbaum found hæmorrhages into the meninges, brain, and mediastinum, a serous exudation into the pleural and abdominal cavities, tumefaction of the spleen, etc. Charbon was suspected, but examination of the blood did not confirm this diagnosis.

EDITORIAL NOTES.

THE ITALIAN SOCIETIES OF HYGIENE will hold their next Congress at Padua in 1890.

A NEW SANITARY CODE has been adopted by the Italian Senate, and will be soon discussed by the Chamber of Deputies.

SMALL-POX is raging to such an extent in many

departments of Italy that the Government has decided to reorganize its vaccination service.

THE RELATIONS BETWEEN ALCOHOL AND RENAL DISEASES is the subject of a debate to be held before the Pathological Society of London on December 4th and 18th.

THE "GAZETTE MÉDICALE DE LIÉGE" is a new French weekly, of 12 pages, the first four numbers of which have been received. It is edited by Dr. Brasseur; assisted by Drs. Merveille and Roskam, and the physicians and surgeons of the hospitals of Liége.

UTERINE TYMPANITES, according to Braun, of Cracow, may be cured by injections of creolin, in a 2 per cent. solution. Braun relates two cases, in one of which labor lasted five days; the fœtus was in a state of putrefaction, and the mother's temperature 40° C., and her pulse 130. In spite of her serious condition the woman recovered.

COCAINE AND OLEAGINOUS SUBSTANCES, when combined, cause a great deal of pain, says the *North Carolina Medical Journal*. In view of the inclination to use vaseline or some such substance with cocaine, especially in operations on the urethra, this is an important point, which possibly has been noted by other observers than Dr. Wood.

THE ALKALOIDS OF COD-LIVER OIL have been investigated by Mourgues and A. Gautier. They describe two fixed bases which accompany the volatile alkaloids: aselline ($C_{25}H_{32}N_4$) from the liver of the *Asellus major*, and morrhuine ($C_{19}H_{27}N_3$) from the liver of *Gadus* morrhua. In the liver of the Gadus there are other interesting compounds.

THE NEW MEDICAL STUDENTS IN LONDON this session number 693, including 25 in the London School of Medicine for Women. The English Provincial Medical Schools, as far as heard from, have 201 students. And yet our British brethren complain of overcrowding of the profession, and that too many are rushing into the study of medicine.

DISINFECTION BY STEAM.—Budde has made some experiments to determine whether the tension and movement of steam have a real influence in disinfection, as Koch states. He finds that Koch is correct; that the destruction of the bacilli of charbon may be considered as certain proof of the efficacy of a method of disinfection. Consequently, anything may be said to be completely disinfected if its deep and less accessible parts have been exposed for at least five minutes to steam at 100° C.

PETERSEN'S BALLOON FOR SUPRAPUBIC LITHOTOMY caused rupture of the anterior wall of the rectum in a case reported by M. Nicaise. An analogous case is reported by Hache. Nicaise's patient was 65 years old, and had already had seven or eight lithotrities performed. The balloon contained 340 grams of water, and the bladder 270 grams.

CHICAGO POST-GRADUATE SCHOOL AND HOSPITAL. — This recently incorporated institution will, it is thought, be opened at an early day. The following are the incorporators and faculty: Drs. N. S. Davis, Sr., J. Adams Allen, H. A. Johnson, A. Reeves Jackson, J. H. Hollister, W. H. Byford, C. T. Parkes, H. T. Byford, Frank Billings, L. L. McArthur, H. P. Newman, W. F. Coleman, F. S. Johnson, Bœrne Bettmann, Josef Zeisler, R. W. Bishop, Frank Cary, J. C. Hoag, Bayard Holmes, G. W. Webster, and Franklin H. Martin.

THE REPORT OF THE VIRGINIA BOARD OF MEDICAL EXAMINERS.—On another page in this week's issue of THE JOURNAL may be found a tabulated statement of the work done by the Medical Examining Board of Virginia from January 1, 1885, to October 9, 1888. It is to be regretted that we have no figures bearing on a larger number of students examined, and on graduates from a larger number of colleges. The figures given, however, are not without value, and we would call the attention to them of all who have any influence in sending students to the medical colleges of this country, and of all directly interested in medical legislation. Physicians of States that have no practice acts should show that table to the members of the Legislatures, and try to induce them to pass an act similar to the North Carolina or Virginia Acts, thus giving the people that greatest safeguard against incompetent practitioners—an efficient Board of Medical Examiners.

DEATH OF DR. HENRY B. SANDS.—Dr. Sands, the well-known surgeon of New York City, died suddenly in his carriage on Sunday afternoon, while returning from a visit to a patient. Dr. Sands was in his 59th year. He was graduated

from the College of Physicians in 1854, and immediately entered Bellevue Hospital as an interne. In 1857 he became demonstrator of anatomy in the College of Physicians and Surgeons, after serving in which capacity he was elected Professor of Anatomy. About ten years ago he was elected Professor of Surgery, his colleague being Dr. Markoe. At one time or another Dr. Sands has been connected with the surgical department of almost every hospital in New York. As a surgical anatomist Dr. Sands had but few equals, and as a surgeon he was successful and brilliant. He was a very rapid lecturer, which detracted somewhat from his value as a teacher. His writing was not very extensive, but all that he wrote was valuable. To see him operate was an unspoken clinical lecture. His death is a very great loss to the profession in this country.

SOCIETY PROCEEDINGS.

Medical Society of Virginia.

Nineteenth Annual Session, held at Norfolk, Virginia, Oct. 23, 24, and 25, 1888.

(*Concluded from page 713.*)

WEDNESDAY—EVENING SESSION.

Returning to the minutes in the regular order, as soon as the meeting had been called to order Wednesday night, October 24, prolonged calls were made by the audience upon DR. HUNTER MCGUIRE, of Richmond. Finally consenting to respond, he made an extempore talk on the

FORMATION OF AN ARTIFICIAL URETHRA FOR PROSTATIC OBSTRUCTION.

(See page 695).

DR. HUGH T. NELSON, of Charlottesville, presented his report as Secretary of the

MEDICAL EXAMINING BOARD OF VIRGINIA FROM THE ORGANIZATION OF THE BOARD, NOVEMBER 15, 1884, TO THE PRESENT DATE.

The Medical Examining Board of Virginia is composed of 32 members of the regular profession; 3 from each of the ten Congressional Districts of Virginia, and 2 from the State at large. These 32 members are nominated to the Governor for commissions, by the Medical Society of Virginia, to serve a term of four years. In addition, the Old Dominion Hahnemann Society nominates 5 homœopathic physicians, who are assigned to committees—thus in reality making the Board composed of 37 members.

Every applicant for license to practice medicine, etc., in Virginia since January 1, 1885, who had not previously secured a license, has been required first of all to receive the permit of this Board before a license could be granted him by any Commissioner of Revenue, Clerk of Court, etc.

The plan for examination of applicants for permits as originally adopted (Nov. 15, 1884) is still in operation, and for thoroughness and fairness has recommended itself to the profession of this State, and to like organizations in other States. Applicants are examined on the eight grand divisions of scientific medicine, namely, chemistry, anatomy, hygiene and medical jurisprudence, physiology, materia medica and therapeutics, obstetrics and gynæcology, practice of medicine, and surgery. The Board is subdivided into eight committees of four members each—each committee having charge of examinations on one of the above-mentioned subjects. In addition, the five homœopathic members who were added to the Board by Act of Legislature, in 1886, were assigned one to each of the following five committees: hygiene and medical jurisprudence, materia medica and therapeutics, obstetrics, practice and surgery.

To each committee is assigned the work of conducting the examinations on a separate subject. Each committee selects and reports the questions in its Section to the Board in session before they are made known to the applicants for examination; and each question thus reported by the respective chairmen of Sections is investigated by the Board, and approved or disapproved—those questions disapproved being remanded to the proper committee for revision or substitution. The standard of requirements adopted by the Board is for the applicants to answer in writing, properly, 75 per cent. of the questions. If an applicant fails to attain of 33⅓ per cent. on any one of the eight Sections, he is refused license, even though he aggregates 75 per cent. as the whole percentage on the eight branches. This was demonstrated to be a necessity from the fact that some applicants entirely ignorned the questions in one Section, trusting to make up the deficiency on other Sections.

Two rejected applicants entered on professional work in their respective counties without the permission of the Board, and in open defiance of the law. Both were indicted and punished by the courts. One of the cases was carried to the Court of Appeals of Virginia, which sustained the Board in every point in which its constitutionality was attacked.

A so-called Resident Physician at one of the Virginia Springs, who was a non-resident of Virginia and who had never stood the examination before the Virginia Board, nor had a license to practice in this State prior to January 1, 1885, was duly notified as to the place and time of the last meeting of the Board in Roanoke, July 17, 1888, but did not put in his appearance. After

this Roanoke session, the Secretary received a letter from this *non-resident* Springs Physician, stating that neither he nor the Manager of the Springs deemed it necessary or convenient for him to go to the trouble of going over to Roanoke to undergo the *formality* of examination, and requested that some special arrangement be made for him. As further evidence seemed to indicate an intention to test the law in such cases, the Commonwealth's attorney of Shenandoah County, Va., has put this doctor under bond for appearance before the October term of his Court. There is no reason why physicians from other States should be granted exemptions from the operations of Virginia laws not accorded our own citizens. Steps are being taken to bring to justice two other doctors practicing medicine illegally in Virginia—one in Powhatan County, and one in Goochland County.

The following is a Tabular Statement of

Work done by the Medical Examining Board of Virginia, from the date (January 1, 1885,) the Law went into operation through the date of the above Report, October 9, 1888.

INSTITUTIONS REPRESENTED BEFORE THE MEDICAL EXAMINING BOARD OF VIRGINIA. (From Jan. 1, 1885, through October 9, 1888.)	Total No. of Applicants from each.	Rejections, Total from each.	Certificates issued by Med. Ex. Board of Va.	No. Rejections making Second Applications.	Second Rejections.	Certificate Issued on Second Application.	Incomplete Examin's by withdrawals, etc.
Medical College of Virginia	57	8	48	3	..	3	1
University of Virginia, Med. Dep't	33	1	32	..	..	..	..
College of Physicians and Surgeons, Baltimore, Md	34	10	24	6	2	4	..
Univ. of Maryland, Med. Dep't, Baltimore, Md.	34	9	25	3	1	2	..
Jefferson Med. Col., Philadelphia, Pa.	12	3	9	2	..	2	..
Univ. of Penn'a, Philadelphia, Pa.	2	..	2	..	..	..	..
Bellevue Hosp. Med. Col., New York.	6	1	5	1	..	1	..
Univ. of City of New York, Med. Dep.	7	2	5	1	1	..	..
Col. of Physicians and Surgeons, N.Y.	3	..	3	..	..	..	..
Louisville Medical College, Ky	2	1	1	..	..	..	..
Hosp. Med. College, Louisville, Ky	3	..	3	..	..	..	..
Kentucky School of Med., Louisville	2	..	2	..	..	..	..
University of Louisville, Ky.	1	..	1	..	..	..	..
Med. Dep't of University of Tennessee, Nashville.	1	..	1	..	..	..	..
Vanderbilt Univ., Med. Dep't, Tenn.	3	1	2	1	..	1	..
Detroit Med. College, Mich.	2	1	1	1	..	1	..
Univ. of Michigan, Ann Arbor, Mich.	1	..	1	..	..	..	..
St. Louis Medical College, Mo.	1	..	1	..	..	..	..
Columbus Medical College, Ohio.	3	2	1	1	..	1	..
Cincinnati Medical College, Ohio	1	1	..	..	..	..	..
Med. Dep't Howard University, Washington, D. C.	7	6	1	4	4	..	..
Leonard Med. Col., Raleigh, N. C.	3	1	2	..	..	..	..
Med. Ded't University of Georgetown, D. C.	1	..	1	..	..	..	..
Hahnemann Homœopathic Medical College, Philadelphia.	2	..	2	..	..	..	..
Medico-Chir. Col., Philadelphia, Pa	3	3	..	2	2	..	..
Geneva Medical College, New York	1	..	1	..	..	..	..
Heidelburg, Germany	1	..	1	..	..	..	..
Baltimore Medical College, Md.	1	..	..	..	..	..	1
Colleges Unknown.	4	..	3	..	..	..	1
Non-Graduates.	11	4	5	..	..	..	2
Cleveland Homœopathic Hosp. College, Ohio.	1	..	1	..	..	..	..
Total number of Examinations	240	54	181	25	10	15	5

Number of Applicants examined by Individual Examiners out of Session. 117
Number of Applicants examined by Board in Session. . . . 123

N.B.—The first and second columns add up 243 and 184 respectively; but three of the applicants each gave two Colleges of graduation.

The following were elected

OFFICERS FOR THE ENSUING YEAR.

President.—Dr. E. W. Row, of Orange, Va.

Vice-Presidents.—Drs. Wm. S. Christian, of Urbanna, George S. Luck, of Roanoke, and L. Ashton, of Falmouth.

Recording Secretary.—Dr. Landon B. Edwards, of Richmond, Va.

Corresponding Secretary.—Dr. J. F. Winn, of Richmond, Va.

Treasurer.—Dr. Richard T. Styll, of Hollins, Roanoke Co., Va.

Committee on Applications for Fellowship at Annual Session, 1889.—Drs. Wm. D. Turner, of Fergusson's Wharf, R. M. Slaughter, of Theological Seminary, I. S. Stone, of Lincoln, Alfred S. Rixey, of Culpepper, and John Clopton, of Williamsburg.

Executive Committee.—Drs. Wm. W. Parker, of Richmond, Hon. Fellow J. Herbert Claiborne, of Petersburg, Jacob Michaux, of Richmond, L. Lankford, of Norfolk, and Hon. Fellow Hunter McGuire, of Richmond. The Recording Secretary and the Treasurer are *ex officio* members.

Committee on Publications.—Drs. C. W. P. Brock, George Ross, and Hugh M. Taylor, each of Richmond, Va. The Recording Secretary and Treasurer are *ex officio* members.

Necrological Committee.—Hon. Fellow Dr. J. Edgar Chancellor, of University of Virginia, Chairman.

To Deliver Annual Address to the Public and Profession, Session 1889.—Dr. Thomas J. Moore, of Richmond, Va.

Subject for General Discussion, Annual Session 1889.—Croupous Pneumonia.

Leader in this Discussion.—Dr. B. L. Winston, of Hanover C. H., Va.

Place of Annual Session 1889.—Roanoke, Va.

Time of Annual Session 1889.—About September 3—the exact day to be definitely fixed and announced by the Executive Committee some months hence.

The next order of business being the nomination to the Governor of

MEMBERS OF THE MEDICAL EXAMINING BOARD OF VIRGINIA

to serve for the regular term of four years, beginning January 1, 1889, Honorary Fellow Dr. J. Edgar Chancellor, of University of Virginia, introduced the following resolution:

Resolved, That the Medical Society of Virginia, desirous of showing their appreciation of the work performed by the Medical Examining Board of Virginia for the term of four years to terminate December 31, 1888, nominate each of the thirty-two regular physicians now representing this Society on that Board, and recommend to the Governor of Virginia to issue commissions in due form to each of those so nominated to serve for the reg-

ular term of four years, beginning January 1, 1889, as members of that Board.

This resolution was unanimously adopted.

The following are the thirty-two members of the regular profession, who are to compose the

BOARD OF MEDICAL EXAMINERS OF VIRGINIA

for the term of four years, beginning January 1, 1889, with their post-office addresses.

State at large.—Drs. T. J. Moore, Richmond, Va.; I. S. Stone, Lincoln, Va.

1st Cong. District.—Drs. S. W. Carmichael, Fredericksburg, Va.; O. B. Finney, Onancock, Va.; W. W. Douglas, Warsaw, Va.

2nd Cong. District.—Drs. Jesse H. Peek, Hamptom, Va.; Herbert M. Nash, Norfolk, Va.; James Parrish, Portsmouth, Va.

3rd Cong. District.—Drs. R. A. Lewis, Richmond, Va.; C. R. Cullen, Richmond, Va.; Hugh M. Taylor, Richmond, Va.

4th Cong. District.—Drs. J. Herbert Claiborne, Petersburg, Va.; W. J. Harris, Blackstone, Va.; Hugh Stockdell, Petersburg, Va.

5th Cong. District.—Drs. Rawley W. Martin, Chatham, Va.; W. L. Robinson, Danville, Va ; T. B. Greer, Rocky Mount, Va.

6th Cong. District.—Drs. H. Grey Latham, Lynchburg, Va.; A. Trent Clark, South Boston, Va.; Oscar Wiley, Salem, Va.

7th Cong. District.—Drs. Wm. P. McGuire, Winchester, Va.; J. H. Neff, Harrisonburg, Va.; Hugh T. Nelson, Charlottesville, Va.

8th Cong. District.—Drs C. C. Conway, Rapidan, Va.; Alexander Harris, Jeffersonton, Va.; Bedford Brown, Alexandria, Va.

9th Cong. District.—Drs. Robert J. Preston, Marion, Va.; R. W. Huffard, Chatham Hill, Va.; S. W. Dickinson, Marion, Va.

10th Cong. District.—Drs. Z. J. Walker, Brownsburg, Va.; H. M. Patterson, Staunton, Va.; G. D. Meriwether, Green Forest, Va.

OCTOBER 25—THIRD DAY.

The Corresponding Secretary, DR. J. F. WINN, of Richmond, presented a circular letter from the Publication Committee having charge of the

REVISION OF THE UNITED STATES PHARMACOPŒIA,

requesting this Society, in common with every other regular State Medical Organization, to appoint a Committee of three to act with the Publication Committee of the American Pharmaceutical Association, in perfecting the revision of the volume to be issued in 1890.

DR. R. M. SLAUGHTER, of Theological Seminary, moved that the Committee of three be appointed as requested—carried: The President appointed Drs. R. M. Slaughter, Wm. B. Towles, of the University of Virginia, and John N. Upshur, of Richmond.

The Treasurer, DR. RICHARD T. STYLL, of Hollins, Va., presented his report for the year just ended, showing a balance on hand of $229.18.

Instead of a formal "Report on Advances in Surgery," to which task he had been assigned, DR. WM. EDWARD MCGUIRE, of Richmond, read a paper on

PATHOGENIC GERMS OR MICROBES, AND SOME CONDITIONS RELATING TO INFECTION.

He said that the "pathogenic micrococci" of chief interest to the surgeon are those of open wounds, such, for instance, as the micrococcus of syphilis, gonorrhœa, acute infectious osteo-myelitis, progressive necrosis, etc. The "pathogenic bacilli" are those of syphilis, tubercle, tetanus, anthrax, ulcerative stomatitis, etc. The baneful influences exerted by these organisms on living bodies have been explained in four ways: Acting as an invading army, they destroy the raw food of the weaker inhabitants (the tissue-cells), which require more complex bodies for their assimilation; or (2) they interfere with the due performance of the depurating organ, and produce disease by preventing the elimination of urea and other final products of repressive metamorphosis; or (3) as microbes, along with all living beings, produce excretion, and this excretion is discharged into the body of the animal in which the microbes live, it would produce deleterious effects as certainly as if the animal's excretions had been retained; or (4), as is most generally accepted, the injurious effects produced by microbes is not due to what they take or excrete, but to what they leave; and this is associated with the demand they make for the oxygen of the tissues. Living and multiplying without direct exposure to air, they obtain their required oxygen from the tissues by which they are surrounded; and when oxygen is withdrawn from such complex chemical compounds as our tissues are composed of, the elements enter into new and abnormal combinations (ptomaines) which are believed to be the really poisonous agents. Depression of vitality of the part attacked is an important factor in the process of infection. An essential condition in the production of local depression is inflammation. Practically, the micro-organisms enter the body in the first stage of inflammation. Much depends on the part of the body exposed to infection—some microörganisms infecting only one part of the body, and others another part. Pettenkoffer believes that everything that increases the amount of water in the blood predisposes the individual to infectious diseases.

Under call for "Reports in Chemistry, Pharmacy, Materia Medica and Therapeutics," the Chairman, DR. R. A. LEWIS, of Richmond,

presented the report, prepared by Dr. Joseph M. Whitfield, lately of Richmond, but now resigned and removed to New York, on

ADVANCES IN CHEMISTRY.

Organic chemistry is the branch attracting special study at present.

Incompatibilities should be better known by physicians. Thus, iodol is incompatible with yellow oxide of mercury, and yet with those unfamiliar with chemistry, such a prescription is often written for an ointment. But in a week's time the neat-looking yellow homogenous ointment changes to a dirty grayish mixture. The trouble is the excess of iodide in the iodol is given off and decomposes the yellow mercuric oxide.

Saccharin, or saccharinic acid, the sweetest of all known substances, belongs to the group of coal-tar products, and is antiseptic. It is claimed also that it acts directly on the fourth ventricle, and thereby diminishes the amount of sugar produced in the system. It is a boon to diabetics having the proverbial "sweet tooth." It is dispensed in half to one grain tablets. One grain will make a cup of coffee too sweet for most persons. It combines readily with most alkaloids, such as quinia, morphia, cocaine, etc. As it prevents fermentation, it is used in gastric and intestinal disorders.

Cocaine saccharinate is about four-fifths the strength of cocaine muriate, but has no bitter taste.

Antipyrin and antifebrin are noted as derived from amilide. Antipyrin is a proprietary drug, and cases of poisoning by it have been reported; but no case of poisoning by *antifebrin* has yet come to light, nor does it interfere with the digestive tract as does antipyrin. Besides, its effects in smaller doses lasts longer; it stimulates rather than depresses the vaso-motor and muscular systems; it has no after effects, and it is very much cheaper than antipyrin. Antipyrin is strongly recommended as a prompt hæmostatic, leaving the wound clean.

Phtalates are salts formed by the action of the organic acid—phtalic acid—on vegetable alkaloids. The special advantages claimed for these salts is their great solubility and stability.

Sulfonal, an oxidation product of ethyl-mercaptan with acetone, is tasteless and colorless and crystallizes in large tablets, easily soluble in alcohol and in two parts of water. It is hypnotic in doses of from gr. xx–xxx.

Amylene hydrate, or di methyl-ethyl carbynol, is a tertiary amylic alcohol—a clear liquid having an ethereal odor and a camphor-like taste. It ranks between chloral and paraldehyde as a hypnotic. It is safer than chloral, as it does not depress the heart or respiratory centres, and has not the nauseous taste nor cause the disagreeable eructations of paraldehyde.

Urethran, or ethyl-urethran, produces sleep like normal sleep, leaves no headache nor gastric disturbances, and only slightly slows the pulse. It is soluble in water and its taste is not unpleasant. Dose for infants from 12 to 18 months old, about 4 grains.

Menthol, or peppermint camphor, is a soluble crystalline salt, looking like Epsom salts, deposited when Chinese oil of peppermint is exposed to cold, forming stearoptine of peppermint oil. It equals thymol as an antiseptic, and hence its use in diphtheria, typhoid and scarlet fevers, etc.

After some notes about cocaine, jambul seed is mentioned as a new treatment for diabetes mellitus. Dose of the fluid extract of the seed, from 5 to 10 minims.

Succinimide of mercury for hypodermic purposes seems to be a valuable preparation. It is a white, silky powder, prepared by heating together ammonia and succinic anhydride, and then adding mercuric oxide. It is very soluble in water and the solution is a permanent one. It is cheap.

DR. H. ROLFE DUPUY, of Norfolk, Va., read the report on

ADVANCES IN MATERIA MEDICA AND THERAPEUTICS.

In reviewing the literature on this subject during the past year, he called special attention to the two great antipyretics, antipyrin and antifebrin, showing their uses and abuses, and the particular indications for each, claiming for the latter the greatest and most varied use. Strophanthus was urged as the great heart tonic, particularly when used in connection with ether, and also with nux vomica. Attention was called to the great value of ichthyol in skin diseases and rheumatic pains. The literature about salicylate of ammonia was reviewed *in extenso* in connection with the treatment of typhoid and remittent fevers. Salol, sulfonal, sparteine, pichi, jambul and other new remedies or preparations came in for their appropriate share of attention. In conclusion, he condemned the use by physicians of proprietary preparations which did not have attached to the description a working formula for the guidance of the pharmacist.

Dr R. C. POWELL, of Alexandria, read a paper on the

ETIOLOGY OF PYREXIA FROM THE STANDPOINT OF MODERN PHYSICS.

He remarked that no theory based wholly on demonstrated science has yet been offered to explain the high temperature in pyrexial diseases. any attempt to explain the phenomenon by a theory not in harmony with the laws of conservation of energy or correlation of forces is not worth consideration. After a full discussion of numerous theories, he presents the following as his conclusion:

1. The matter of the human body is identical with that of the world around us; and the forces of the human body are the same as those of inorganic nature. This body as a piece of mechanism are more perfect than any other apparatus for the transformation and distribution of energy, with which it is supplied, but possesses no creative power.

2. The relation between combustion and heat is coincidental, and not causal. If heat is the result of collision between carbon atoms and oxygen atoms, this collision must precede that intimate union of such atoms which is termed combustion; and as the effect can never precede the cause, heat cannot possibly be the effect of combustion.

3. The oxidation of tissues *cannot* be exaggerated by the presence of micro-organisms whose very existence is maintained by the *absorption* of oxygen from these tissues.

4. In all *essential fevers*, the pyrexia is probably due to increased molecular motion.

5. The rapid waste of fatty tissue in fever is not so intimately connected with the production of heat as with the production of energy—some of which is manifested in the accelerated action of respiration and circulation.

6. In sympathetic pyrexia, heat is a form of energy which, by reason of traumatic lesions of the nervous system, is prohibited from expending itself in functional activity or motion, and is therefore transmuted into heat, which is the form most frequently assumed by transmutable energy.

7. The only rational way to treat essential fever is to destroy the cause of it; but for agents to cause this destruction, no enthusiastic search will be made until we repudiate the idea that the great object in the treatment of fever is to place the system in the most favorable condition for recuperation after the disease shall have run its course.

Dr. LEWIS G. PEDIGO, of Roanoke City, read a paper entitled

ANTAGONISM BETWEEN AMYL NITRITE AND PRUSSIC ACID.

After certain preliminaries, he gave an account of three representative experiments out of a series that he had performed on lower animals, illustrating the use of amyl nitrite by inhalation as a treatment for prussic acid poisoning. The experiments consisted in administering the poison in large doses—hypodermatically—and beginning the use of the antidote at once by inhalation. Collateral experiments were reviewed in which the poison was given in equal or smaller doses and the antidote withheld. The effects were observed and compared, always showing a remarkably heavy balance in favor of the remedy. In one of these experiments a dog's life was saved by the use of the nitrite of amyl from the action of the same dose which had just killed a similar dog in five minutes and fifteen seconds, without treatment. In commenting on the experiments, a theory was evolved to explain the action and antagonism of the two drugs. The convulsion and arterial spasm of prussic acid poisoning were attributed to paralysis of the inhibitory nerve centers. The relief of these symptoms by nitrite of amyl was accounted for on the supposition that this drug stimulates those centers. The two drugs were shown to antagonize each other in six distinct physiological items, thus proving the amyl to be a more nearly perfect phsiological antidote to prussic acid, than is known for any other poison. He suggests the use of the same remedy in cases of poisoning by other cardiac depressants, and enumerated aconite, veratrum viride and gelseminum.

(*To be concluded.*)

Obstetrical Society of Philadelphia.

Stated Meeting, October 4, 1888.

THE PRESIDENT, T. M. DRYSDALE,, M.D., IN THE CHAIR.

(*Continued from page 679.*)

DR. B. F. BAER read the following report of

TWO CASES OF MULTILOCULAR OVARIAN CYSTOMA

of unusual size and very rapid growth. Mrs. T. was sent me by Dr. J. A. Clark, of Bedford, Pa., and on July 31st entered my private hospital. She is æt. 28 years; married; has had two children after normal labors, the youngest being six years of age. About nine month previous to this date, she was attacked with severe pain in the right ovarian region and was confined to bed for several weeks. Her menses had always been regular, but at this time the flow was profuse and continued two weeks. Soon after this attack of pain and metrorrhagia she noticed a swelling in the painful region, on the right side. She rapidly increased in size and began to lose flesh, and occasionally to have attacks of pain and metrorrhagia similar to the one noted above, the flow for several occasions lasting for a month. Her abdomen was enormously distended, especially in the upper portion. It was rather symmetrical, dull on percussion all over the anterior and lateral portion except in the lumbar regions, where slight resonance was observed. There was fluctuation in the lower part, but in the upper portion it was very obscure. The skin on the lower surface of the abdomen was in a condition of elephantiasis.

Vaginal examination revealed the uterus slightly retroverted, rather mobile, and gave a sound measurement of three inches. The lower surface of the tumor could just be felt by the vaginal examination. The patient had a very weak pulse, indeed it could not be felt at all at

the left wrist and she had great dyspnœa on the slightest exertion.

Operation was performed on August 2nd. I was assisted by Drs. J. C. Bowen, G. H. Franklin, J. A. Clark and H. C. Bloom. An incision three inches in length was made in the usual position. The skin at the point of the incision was fully half an inch thick and very vascular and considerable subcutaneous adipose tissue was present. As soon as the tumor was reached it presented the appearance common to ovarian growths, but it was found to be closely adherent to the abdominal wall. After separating as far as the finger would reach, the tumor was punctured with Tait's large trocar, and about four gallons of greenish fluid drained away; but only the lower portion of the tumor collapsed, the greater and upper portion remained as before. This was punctured in a number of places without removing the instrument from the cavity which had been drained, but nothing followed. The opening in the tumor was now enlarged and the hand introduced and the multilocular condition broken down, large pieces of semi-solid substance being torn loose from the cavity of the tumor and brought away, together with a great deal of semi-fluid debris. As soon as room was gained the hand was carried outside the tumor, when it was found to be adherent to the liver, stomach, and everything with which it came in contact. These adhesions were carefully separated and after considerable effort the remainder of the tumor was finally brought out through the incision, which had been previously increased to three and a half inches. The pedicle, which was found to be thick and quite vascular, was transfixed and ligated and the tumor cut away. The cavity was irrigated with filtered boiled water, as much as two gallons being used. The irrigating tube was carried in every direction until the water returned clear. The right ovary was not removed, it being in apparent health. The wound was closed around a drainage tube. There was considerable shock. The pulse could not be felt at either wrist and it was two days before it returned, although the patient seemed to be doing well. The usual after treatment was carried out and the patient has made an excellent recovery. She went home (250 miles) on the twenty-seventh day and still remains in good health. The temperature never rose above 100° and the drainage tube was removed on the second day. The tumor was a multilocular cyst and weighed about ninety pounds.

On September 5th I was asked by my friend Dr. R. Armstrong, of Lock Haven, to meet him in consultation in a case of abdominal tumor, which he stated was in such extreme condition that he feared she might not live until my arrival.

I saw the patient on September 6th. She is 21 years old and single; puberty had occurred at twelve and menstruation had always been profuse, coming on every three weeks and always attended with some pain; she did not consider this abnormal, and so far as she knew was perfectly well up to four months previous to the above date. In the latter part of April of this year after unusual exertion about the house, she was suddenly attacked with severe cramp-like pain in the right iliac region, so severe indeed as to alarm the neighbors by her outcries. This attack occurred about the time of her expected menstruation and continued until the flow followed, when she gained considerable relief. But she remained ill from that time, being able to go about, however, in the intervals between the series of attacks of pain of similar character which now followed. Within two weeks after the first attack she noticed that her abdomen was increasing in size in the painful region and from that time to the present, just four months, her abdomen has grown to an enormous size. I found her occupying a semi-recumbent posture and breathing with difficulty. She was emaciated to such a degree, and the tumor was of such size, that she was almost hidden from view beneath it. The surface of the abdomen was purple from interference with the capillary circulation and the veins were greatly distended. The abdomen was symmetrical and smooth. Fluctuation was rather obscure. There was dulness on percussion all over the anterior and lateral surfaces of the tumor, except at a point far back in the left lumbar region, where slight resonance was found. On the upper right border of the tumor, in the region of the liver, there was an apparently solid mass, shaped somewhat like the liver, suggesting the possibility that the cyst had grown from that organ. This was given more prominence on account of the rapidity of the growth. The patient was unable to retain anything on her stomach; she had not slept except at short intervals, for weeks. Her bowels were constipated and the urine was passed frequently and in small quantities. Her pulse was 140 and very feeble. Her expression was an appealing one and she begged to be relieved.

A tablespoonful of whisky was given and repeated in two hours, just before the administration of the anæsthetic. I was ably assisted in the operation by Drs. Armstrong, Ball, and Watson, of Lock Haven. An incision two inches in length was made. The surface of the cyst was adherent to the peritoneum. After separating the adhesions as far as I could I plunged a large trocar into the tumor. But the contents were semi-solid. I therefore cut through the cyst-wall and proceeded to break up and remove the contents. The cyst was adherent to everything it touched, liver, stomach and other viscera, but the adhesions were weak, and in ten minutes' time the tumor was removed and the pedicle, which was thick and vas-

cular, was ligated. The omentum was so firmly adherent to the cyst that it was ligated and amputated. The friable cyst-wall was ruptured in many places and a great deal of the viscid semi-fluid material escaped into the abdominal cavity, but I did not lose time in trying to prevent this. When the tumor was removed what was left of the patient was an exceedingly small portion. The emaciated abdominal walls lay close to the spinal column and sunk into the pelvis. She looked more literally "nothing but skin and bones," than anything I had ever seen before. The abdominal cavity was thoroughly washed out by irrigation through a fountain syringe, and I was careful to pass the nozzle high up among the intestines and under the surface of the liver and diaphragm. The water returned clear and the incision was closed around a drainage tube and the patient returned to the bed with a better condition of pulse and appearance than she had before the operation. She did not show any evidence of shock and was conscious almost as soon as she was placed in bed. Her body was so emaciated that it was necessary to pack with cotton about the pelvis and along the spinal column, as the bones almost projected through the skin, and at several places bed-sores were apparent. The R. ovary seemed smaller even than its natural size and appeared thinner; it was therefore not removed. The after history of the case has been without event. Her temperature never rose above 100° and was normal on the third day after the operation. The pulse gradually diminished from 140 and was normal on the fifth day. The drainage tube was removed within thirty-six hours after operation. The sutures were removed on the eighth day, when union was found complete, except at the lower portion where the drainage tube had been, and this has since healed. She began taking solid food on the third day and on the fourth day her bowels were moved. The tumor weighed about 75 lbs.

The points of considerable interest in these cases are the location, character, and severity of the early symptoms, as well as the location of the tumor when first noticed (in the right side), while the tumors were of the left ovary, the right being perfectly healthy; the large size and very rapid growth of the tumor; the rapid recovery of the patients although in extreme condition, especially of the case last mentioned; the fact that the two cases are alike in nearly all particulars, the only difference being that in the second case the rapidity of the development was much greater, and the severity of the symptoms likewise greater; and lastly, the method of removal of the tumor, that is, the breaking up of the semi-solid contents with the hand, thereby permitting their removal through a very short incision. I wish here to call attention to a fatal case which occurred in my practice several months ago and which forcibly illustrates that there may be danger in introducing the hand for the purpose of breaking down contents of the tumor, not knowing exactly the location of the intestines. In the case referred to the friable wall of the main cyst had ruptured and some coils of intestines were found to be in the cavity and closely adherent to the more solid portion of the contents. Very careful manipulation was necessary to separate the bowel, which was finally done after considerable time had been spent in the effort. Ordinarily, however, where the cyst has not previously ruptured the procedure is a safe one when due care is observed.

DR. M. PRICE reported a case of

PYOSALYNX WITH RUPTURE.

On the 6th of September I was called to Mrs. ——, with symptoms of miscarriage, with pains, hæmorrhage and slight odor to the discharge. She refused to have an examination, saying she knew she was not pregnant. I left her with the understanding that when she was ready for me to examine her to send for me. On September 10, I was again called, and found her in great pain; the discharge of blood and broken down placenta were of the most offensive character. She stated that she had been perfectly regular up to her last period, which was delayed about one week. She had considerable fever, a temperature of 102°, and had had that morning a severe chill. On examination the uterus was found about four inches in depth, with part of a rotten placenta adherent to its right posterior wall. The uterus was in good position and perfectly movable, with both tubes enlarged and thickened, and at this time could not have been adherent to any surrounding structure. I removed the placenta with considerable difficulty, used hot water irrigation with boric acid in the uterine cavity, which, for a time, gave her great relief. These irrigations were continued, and the uterus washed out twice a day for three days, all this time the tubes continuing to enlarge, until they must have contained several ounces of matter, and could have, at this time, been easily removed. The irrigation into the uterus were discontinued, and those of the vagina were kept up. I became very much alarmed at her condition, and stated to the husband that an operation was needed to save his wife's life. This he refused, and begged that I should do all I could without the operation. I yielded to his request much to my regret, for I felt that nothing but an immediate operation and removal of the tubes, which then would have been possible, as there had been little if any leakage up to that time into the peritoneal cavity, would save her life. I believe that any man treating a case of this kind with the symptoms as positive and the indications as plain as they were in this case for operation, should have retired from the case, for by so doing, he clearly indicates that his mind is

made up as to the treatment and the only chance to save, and by so doing, shows to the medical attendant who may be called to the case, the proper line of treatment, and if he does not take the warning, the post-mortem will follow and show who was right. There were several well-marked changes in her condition, indicating rupture or leakage from the tubal abscess, and her condition steadily grew worse until the 20th, when in consultation with my brother, we persuaded the family and the patient to let me operate and give her that chance for life, as she was in a very bad septic condition. As the consultation was at a very late hour at night, she was opened early the next morning (21st). I found the internal organs matted together, uterus much enlarged, both tubes enlarged and ruptured, adherent to everything they touched, pelvis full of pus cavities, pus cavities almost up to the kidneys on both sides; everything in a semi-gangrenous condition; but little bleeding from ruptured adhesions or from wound in opening abdomen, which is never a good indication. A great quantity of pus was evacuated, at least two pints, of the most offensive character. Irrigation and drainage were used. The patient was a very large woman, consequently the longest drainage tube we could find was used. She rallied from the ether, and for the first six hours there was discharge from the drainage tube, two pints of very offensive serum. It gradually lessened in quantity, but increased in offensive character. A cleaning of the tube was made every half hour; before cleaning warm boracic water was injected through the tube. It improved matters only for the moment. Patient died twelve hours after the operation. Present at the operation: Drs. Joseph Price, E. W. Cushing, of Boston, Atherton, Toronto, Roseburg, Hamilton, Ontario.

DR. W. H. PARISH said that his remarks on this subject of pelvic abscess read at the recent meeting of the American Gynæcological Society had been misquoted. He had stated there, and wished to repeat here, that these abscesses should be opened very early. If operation was not resorted to the patient would most probably either die or become a confirmed invalid. He was not one of those who believe that pus always originates in one particular point in the pelvis. He did, however, believe that the large majority of cases occur because of pus primarily in the tube. He believed, also, that an uncertain number of cases occur from pus originally formed in the areolar tissue, beginning probably because of lymphangitis of that particular locality. The question arises as to how best to operate in these cases. He said that there could be no absolute rule of procedure. He believed that, in the majority of cases, it was wiser to make an opening in the median line and explore the peritoneal cavity, unless we are very certain that the abscess is not in the tube or ovary. If we are sure that there is no involvement of the appendages and that the pus is not intraperitoneal, the abscess may be opened without going into the cavity. He called attention to a procedure which he had adopted in a few instances, where small abscesses were located in pelvic areolar tissue. In one instance Dr. Longaker made an incision in the median line. The tubes and ovaries were found free from pus, but of course congested. With the fingers within the abdomen he felt in the anterior pelvic wall an abscess. An incision was made over Poupart's ligament as for ligature of the external iliac. Then passing deep into the pelvis, pus was reached some distance below the brim of the pelvis. In another case there was an indurated mass apparent above the left half of the pelvis, not very recognizable through the vagina, except on very deep pressure. An incision was made above Poupart's ligament. After cutting through very dense tissue, he came to a minute cavity which contained no pus, but a somewhat serous fluid containing flakes of lymph. These are only two of a considerable number of pelvic abscesses in which he had operated, and he had never regretted operating early.

DR. J. M. BALDY wished to take this opportunity of emphasizing views which he had expressed before the recent meeting of the American Gynæcological Society. He did not agree with Dr. Parish as to the pathology of this affection. He granted that there was the possibility of an abscess occurring in the pelvis, such as occur in other parts of the body from the scalp to the foot, but that these must be most rare. The gentlemen connected with what Dr. Parvin had been pleased to call "the Philadelphia Dispensary School of Surgery" had now done over one hundred of these operations, and had not yet in a single case come across one which had not begun primarily in the tubes or ovaries. In every case the diseased mass removed has been tube, ovary and other tissues involved. (Dr. Parish, at this point, asked wherein his views differed from those of Dr. Baldy?) He had to leave that to be inferred from what Dr. Parish had said. In regard to treatment he must again dissent from the views expressed. He thought that an absolute rule *could* be laid down. Where pus was found in the pelvis, early or late, the proper procedure was to open the abdominal cavity, and where it was not possible to remove the seat of the disease, proper drainage should be established. However, it would be found comparatively seldom that the abscess could not be taken out by a bold operator.

DR. JOSEPH PRICE thought that he understood Dr. Parish. He himself had said repeatedly that we might have an abscess in any part of the body from the scalp to the matrix of the nails, we may have it in the cellular tissue of the pelvis as well as in the axilla or neck, but he must hold to

what he had said, that in all the pelvic abcesses that he had seen he had not found one not due primarily to tubal disease. Among the recent papers on the subject, one calls attention to the treatment by drainage through the vagina. He did not see how this will avail much in bilateral accumulations. You may evacuate half of the tube, but you have left a condition of affairs such as is found in an old bubo. In pelvic abscess we have just the condition of affairs which the surgeon is asked to treat in the groin, axilla or popliteal space. In such a case he would remove the disease by a clean enucleation and perfect a cure. He had not seen a case of pelvic abscess which could not be removed in this way and he should say that such cases did not exist. One gentleman at Washington went so far as to say, that after drainage by vagina in a case of double pyosalpinx, recovery had followed—the woman had borne children. He might as well have said she had conceived, notwithstanding her husband had previously been castrated.

DR. M. PRICE remarked that it was a question whether you could say that the tube was diseased or not, by simply looking at it. He remembered a case a few weeks ago, where the tube was congested and inflamed. It seemed to be simply swollen, but on pressure there was forced from the fimbriated end a drop or two of as perfect gonorrhœal pus as could be found anywhere. If he had not seen the discharge, he should have thought that there was no disease save congestion.

(*To be concluded.*)

FOREIGN CORRESPONDENCE.

Pettenkofer's Views on Zymotic Diseases and Quarantine.

[The following is translated from manuscript given by Professor von Pettenkofer to Dr. Horatio R. Bigelow, for THE JOURNAL.]

For thirty years I have held the opinion that all the so-called zymotic infectious diseases (typhoid fever, cholera, yellow fever, etc.) are caused by lower organisms, and that the specific germs are spread by human intercourse. I differ from the contagionists, however, in the important particular that I place the causes of the epidemic diseases, so far as they show a striking dependence upon locality and season, not in people, but in their surroundings.

I classify infectious diseases as entogenous and ectogenous: entogenous when the virulent infectious material is simply transferred from the infected to the healthy person, and infects him (such as small-pox, syphilis, etc.); ectogenous when the germ spread by human intercourse must first go through a certain stage outside the human organism in order to increase and become virulent, and become capable of infecting, as for example malaria.

It is not the cholera patient nor the yellow fever patient that infects, but the cholera locality and the yellow fever locality. Physicians and nurses, therefore, are no more affected than persons who do not come at all in contact with the patients. In a hospital containing cholera or yellow fever patients, but which is not a cholera or yellow fever locality, the nurses and attendants remain healthy, and other patients are not infected. Of course a hospital, just as well as other houses, may become an infected locality, and in that case attendants and other patients may become infected; a so-called house epidemic breaks out, but in this case the infection is due to the locality, not to the patients. So it is in barracks in which epidemics break out without a sick person having been previously brought there.

House epidemics in hospitals in which cholera patients are assembled are not more frequent nor more severe than house epidemics in barracks in which every suspicious case of diarrhœa among the soldiers is immediately removed—isolated.

For infection there must be not merely the presence of infectious material, but there must be also a certain quantity of it. It is true, so much locally produced infectious material may be carried from cholera and yellow fever localities, in individual cases, by human and other means, into other places, that infection is produced; but this seldom occurs, and does not cause epidemics, but sporadic cases only, when the germs do not fall on fruitful soil, and the place is not disposed to the ectogenous spread of infecting material.

There are places that always have immunity from imported infectious material (such as Versailles, Lyons, etc.), but the affected places have immunity from ectogenous development of the imported germ, which alone is capable of causing infection, only temporarily and now and then.

As in places in which cholera or yellow fever is endemic there is often a considerable length of time in which there are no patients, or they are very few, without the specific poison having disappeared from the place, and then again there are numerous cases after a little time, without the infectious material causing them having been newly imported, so may the specific material from cholera or yellow fever localities, carried to other places by human intercourse, cause an epidemic after it has had time for development (which time, according to Pettenkofer's observations, may be many months).

For this reason we seldom find the trace of personal or local connection between the first cases in a country or in an active commercial city, as well between one another as with immediately preceding foreign cases, or cases brought from without.

For this reason also all quarantine regulations of land and water, all disinfection and isolation,

which are limited to the known cholera or yellow fever patients, are always ineffective.

Pettenkofer cites many facts going to show that cholera epidemics do not become more violent and do not last longer when intercourse with cholera patients is not limited to the least extent, and when neither isolation nor disinfection are practiced. Where these measures seem to have been used, the local and temporary disposition of the locality was wanting. So soon as there is this disposition in a place, and the specific material is present, the disease breaks out, and all measures based on the grounds of the contagionists are futile.

In order for the disease to occur there is necessary not only a sufficient quantity of the specific infectious material, but also an individual disposition. When a number of people are exposed to the same influences of intercourse and infected locality, all of them are never attacked, but only a small number, corresponding to the more or less developed individual disposition. The duration (?) of the disease —even in a slight degree—protects for a considerable time against repeated infection.

Some are inclined to explain the temporary outbreak and the gradual subsidence of a cholera epidemic in a place by the existence of individual disposition and its subsequent exhaustion. This is a mistake, however, for were it true there would be no places having immunity, whose inhabitants could pass through the disease, and after the introduction of the poison severe epidemics would certainly break out. Nor can the extinction of the local epidemics be explained on the theory that that there are no more disposed persons in the place. The explanation must be that the place ceases to produce virulent infectious material. When this happens disposed persons come in greater number from without, and the epidemic is extinct (Leipzig, 1866, and Messina, 1887).

The occurrence and conduct of cholera on board ships, in the light of later examination, are very much against the contagionist and more in favor of the locality views.

The drinking-water theory is applicable neither to cholera, nor to yellow fever, nor to typhoid. In non-sterilized water cholera bacilli and typhoid bacteria perish quickly, partly for want of suitable nutritive material, and partly also from coming in contact with the non-pathogenic water bacteria; and even when they are viable in a cholera or typhoid stool in wells or water-pipes, the infectious material is so extraordinarily diluted, that infection from the use of such water cannot occur. Hitherto there has been no experiment on animals that has succeeded in infecting one of them with pathogenic micro-organisms (of anthrax, chicken cholera, etc.) through drinking-water; and this is in complete accord with epidemiological facts. Nevertheless, Pettenkofer is enthusiastic on the subject of good drinking-water, because clean and pure water is generally a deeply felt want, and is a necessity not merely in typhoid and cholera places, but in all other places.

As regards the prophylactic measures, these should be directed: 1. Against the spread of the germs of specific disease by human and commercial intercourse. 2. Against the individual disposition. 3. Against local disposition.

The spread of the specific germ can be hindered only by the discontinuance of all personal and commercial intercourse with infected places, but never by a mere watching and regulation of such intercourse, since this is never, in the bacteriological sense, fungus-proof. As a rule the germs are already spread before a patient is discovered in a place. All the rules in this direction have, therefore, been without good result, whether applied to land or water. The cessation of all intercourse for a long time, for a series of years, even were such a thing possible, would be a greater misfortune than cholera and yellow fever.

It would certainly be better if people could be freed from the individual disposition, or even if this could be lessened, as in the case of small-pox by vaccination. It is possible that an inoculation material may be found for cholera, but thus far none has been found to serve the purpose.

We have well-tried measures against local disposition. The most important part of the local disposition is uncleanliness of the soil due to human excreta. Cleansing and keeping clean the soil upon which our dwellings stand is the best prophylactic measure against cholera and typhoid epidemics, and probably also against yellow fever. Wherever there is proper house-drainage and removal of every kind of excrement and garbage by canalization, removal of all privy vaults, and of all material that will not float, the disposition to these diseases is either entirely destroyed or very much diminished. England, for example, in spite of its unbroken and very free communication with cholera-infected places, and in spite of several important cases of cholera, since 1866, in consequence of its sanitary improvements, it has had no epidemic of cholera. The City of Munich, formerly known to the whole world by reason of its numerous cases of typhoid fever, has now only 14 cases per 100,000 yearly, instead of 300 per 100,000 as in former years. There are places so fortunate as to have a natural immunity against cholera. But even those places that have not natural immunity, can be rendered free of disposition by hygienic art (such as Fort William at Calcutta).

But the measures for giving immunity to a place should not be delayed until an epidemic breaks out, but should be carried out beforehand, so as to be truly prophylactic. An unclean soil cannot be made clean suddenly, even when man ceases to pollute it further, but it requires time for self-cleansing; just as a field does not become

suddenly unfruitful when we cease to fertilize it. It requires time, and more or less according to the quality of the soil.

These and many other important epidemiological facts are laid down in Pettenkofer's writings. The most important are to be found in his two recent works: "Zum gegenwartigen Stand der Cholerafrage," Munich, 1887, and "Der epidemiologisches Theil der Berichtes über die Thätigkeit der zur Erforschung der Cholera im Jahre 1883 nach Aegypten und Indian entstanden deutschen Commission," Munich, 1888.

Local factors are more prominent in yellow fever than in other infectious diseases. In yellow fever the local limitation is much narrower. The epidemics usually occur on the seashore and on the banks of large rivers, and do not extend far inland. Temperature, brakish water, or soil water containing a good deal of salt, in the soil has a local influence.

The researches of Professor Billings, of the University of Nebraska, in regard to cattle plague or Texas fever and yellow fever show many analogies to the views of Pettenkofer.

NECROLOGY.

Henry B. Sands, M.D.

HENRY BERTON SANDS, probably the best known of the surgeons on the Atlantic seaboard, died of apoplexy, in his carriage, November 18, with an apalling suddenness. He was born Sept. 27, 1830, in New York City, and was throughout life identified with it as a resident. His father, also a native of the city, was long and favorably known as one of its most trustworthy apothecaries. He was from the very beginning of his career an earnest and enthusiastic student, with somewhat of an outside bias for instrumental music, having in early life even filled the position of organist in one of the leading churches.

Dr. Sand's name was long associated with the New York College of Physicians and Surgeons as Demonstrator of Anatomy, Professor of Anatomy, and Professor of Surgery. Besides these positions, he was connected with nearly all the hospitals of that city, either as Visiting or Consulting Surgeon. His society membership was also extensive.

As an operator, Dr. Sands probably had no superior as a rapid, graceful and skilful manipulator—these qualities added to an almost unerring judgment and a certain diagnostic instinct, gave him a fame, which out-leaped the barriers of his home. His success as a laparotomist was almost unique, and there were but few cases of appendix vermiformis disease in his native city or environs which were not in some way brought to his notice.

Of a marvellous industry, and unimpeachable punctuality conjoined with a devoted loyalty to his profession, and an ever-present modesty, he was a favorite consultant with his brethren far and near. His reputation certainly rests upon a very secure basis.

MISCELLANEOUS.

A DIPHTHERIA SCARE prevails at Alden, Iowa, and the town schools have been closed.

DR. D. HAYES AGNEW will, it is reported, soon resign the Chair of Surgery in the University of Pennsylvania.

CONGENITAL DEFECTS.—Dr. R. R. Williams, of Manning, Ia., informs us that in October, 1888, near Manning, a child was born with but *one* hand and wrist.

BAD MEAT IN OMAHA.—The Omaha Meat Inspector's report shows that 3,485 pounds of diseased meat and six gallons of oysters have been condemned during the past month.

NORTH CENTRAL ILLINOIS MEDICAL ASSOCIATION.—The fifteenth annual meeting of this Association will be held in the M. E. Church, LaSalle, Ill., on Tuesday, December 4, 1888, at 10:30 A.M.

THE SOUTHERN ILLINOIS MEDICAL ASSOCIATION met at Duquoin on Nov. 15. On the second day, after an interesting and profitable meeting, the Association adjourned to meet in Metropolis on June 15, 1889.

DEATH OF AN ALLEGED CENTENARIAN.—Mrs. Hannah Sharkey, the oldest woman in Ohio, died at Youngstown, Ohio, a few days ago. She was said to have been 111 years old, having been born in Cork, Ireland, in 1777.

UTICA MEDICAL LIBRARY ASSOCIATION.—The following are the officers for the ensuing year: President, Dr. J. G. Kilbourn; Vice-President, Dr. Chas. J. Wagner; Secretary, Dr. D. C. Dye; Treasurer, Dr. H. Quin.

SOUTH KANSAS MEDICAL ASSOCIATION.—The twenty-sixth semi-annual meeting of the South Kansas Medical Society met in Hutchinson on November 13. There was a large attendance of leading physicians from all over the southern portion of the State.

SCARLET FEVER.—A dispatch from Jamestown, Dak., says that an epidemic of scarlet fever is raging there. Churches, Sunday-schools and public schools have been ordered closed during the prevalence of the disease. A number of deaths have already occurred.

A PROBABLY FATAL ACCIDENT to Dr. Nathan Rogers, of San Francisco, occurred on November 2. His horses ran away, overturning the carriage and throwing Dr. Rogers out. His head struck the curbstone, resulting in a fracture of the skull which is pronounced fatal.

CREMATORIES.—At the congress of the advocates of cremation recently held in Vienna it was stated that there are throughout the world fifty crematories, half of which are in the United States, twenty in Italy, and one each in Germany (Gotha), England, France and Switzerland.

A SCHOOL OF DENTISTRY FOR NEGROES, we learn from a Chicago newspaper, has been established in Nashville, probably as a department of the Meharry Medical School. The Meharry School and Leonard Medical School, of Raleigh, N. Ca., are doing good work—better than the majority of medical schools for white people.

LEPROSY IN DAKOTA.—There is a case of leprosy at Harold, Hughes county, Dak. The subject is the child of Mrs. Bausum, who was a missionary to China when the child was born. The case was examined in New York a year ago and pronounced to be one of leprosy. The members of the afflicted family are allowed no communication with the neighbors.

POWER OF THE IMAGINATION.—DR. DURAND, wishing to test the effects of the imagination on health and disease, experimented on a hundred patients, to whom he gave a dose of sweetened water. Fifteen minutes after he entered, apparently in great excitement, and announced that he had made a mistake, having administered a powerful emetic, and he directed that preparations should be made accordingly. Eighty out of the hundred patients were thoroughly ill, and exhibited the usual results of an emetic.

ARCHIVES OF GYNÆCOLOGY.—Seven hundred and twenty-eight is the record in numbers of the articles printed during 1888 in the *Archives of Gynæcology* on the special subjects of its title. It is the aim of the editors to publish all current thought in these departments of medical knowledge. The publishers, Leonard & Co., 141 Broadway, New York, do not send sample copies, but if you are not pleased with the first number it may be returned and the order erased. Subscription $3.00 per annum. Payment is not asked till end of the year.

THE CAUSE OF ILL-TEMPER has been discovered by a dress-reform lecturer, who does not consider it an evidence of bad disposition. She says that "oftener than not it is owing to bodily discomfort proceeding from improper dressing, and that wives would all be sweet-tempered if they were properly attired and took sufficient exercise." There is probably a good deal of truth in this. Improper and uncomfortable dress and indigestible food are sufficient to cause ill-temper; and if ill-temper be given the rein while food is taken indigestion will result sooner or later.

"ELECTRIC PROSTRATION" is the name given to a disorder which troubles workers under electric lights. Severe cases are reported from Creusot, France, where an electric furnace is used for quickly heating metals. The light exceeds 100,000 candle power, and the men suffer from it, not from the heat. After one or two hours the workers have a painful sensation in the throat, face, and temples, the skin becomes copper red, and an eye irritation begins that lasts forty-eight hours, the discharge of tears being copious. After five days the skin peels off. Dark-colored glasses somewhat mitigate the effects of this tremendous light, but not entirely.

CONSULTATION BY TELEGRAPH.—The Canadian Pacific Railway Company's telegraph was on Nov. 18 brought into service in a way that not only afforded a good illustration of the extent of the system, but furnished a unique example of the possibilities of modern science. Lord Ennismore, heir to the Earldom of Listowell, is lying at the point of death in the hospital at Victoria, B. C., with typhoid fever. Through the aid of Sir Donald A. Smith, who is now in London, a telegraph circuit was formed from London to Victoria by the Mackay-Bennett Cable and Canadian Pacific Railway telegraph, and Sir Andrew Clarke, the distinguished London physician, was placed in direct consultation with Dr. Hannington in Victoria. A conversation lasting three hours, concerning Lord Ennismore's condition, was carried on. An unbroken circuit was worked from Victoria to the cable office in New York, where the telegrams were repeated to London. Replies were received in three and four minutes.

HOMICULTURE.—A writer in the *Nineteenth Century*, taking the cue of the impulse to discuss human culture, makes some suggestions under the heading "Homiculture." He believes a good deal could be done by directing public attention to the laws that underlie the improvement of stock. He also urges that before a marriage permit is allowed parties should be compelled to submit to a medical examination, to show that they are not laboring under hereditary diseases. Beyond this he would have the absolute prohibition of habitual criminals from propagating their kind. These suggestions have been to some degree urged before by Dugdale, but they are timely. No question is of more importance than the improvement of humanity. We have traced our ills to heredity. Our remedies must correspond. Probably all suggestions at present will be crude and premature, but it is nonsense to suppose an evil exists without a possible cure.

DIPHTHERIA CARRIED BY A TURKEY.—A fowl with diphtheria was brought to the house of a veterinary surgeon on April 24, and died on the 29th. The feeding and nursing of the bird devolved on a lad aged 14, who was assisted by his brother, aged 5. On the evening of May 11 the writer was called to see the little boy of 5, who had been poorly for a day or two. He had enlarged cervical glands on the left side, which had come on rapidly. He was a delicate little fellow, with fair hair and anæmic aspect. The fauces were more or less covered with diphtheritic membrane, the left tonsil more especially. Under the administration of biniodide of mercury and iron the throat symptoms cleared up and the child made a good recovery. On the day after this case was first seen the boy who fed the fowl was very feverish and had similar patches over his fauces, but not to the same extent as his brother. A sister, aged 9, had also a similar explosion on the fauces. On the 18th the mother, who nursed them, was attacked and was similarly treated. They were all kept well up with beef-tea and stimulants.—*British Medical Journal.*

Official List of Changes in the Stations and Duties of Officers Serving in the Medical Department U. S. Army, from November 10, 1888, to November 16, 1888.

By direction of the Secretary of War, the retirement from active service this date, by operation of law, of Col. Elisha I. Baily, Surgeon, under the provisions of the Act of Congress approved June 30, 1882, is announced. Col. Baily will repair to his home. Par. 1, S. O. 266, A. G. O., Washington, October 14, 1888.

By direction of the President, Lieut.-Col. Basil Norris and Major Henry R. Tilton, Surgeons U. S. A., are detailed as members of the Army Retiring Board appointed to meet at San Francisco, Cal., by War Department order dated October 27, 1888, published in S. O. 253, October 30, 1888, from Hdqrs. of the Army, vice Col. Elisha I. Baily, Surgeon, and Capt. John J. Cochran, Asst. Surgeon, hereby relieved. Par. 13, S. O. 261, A. G. O., Washington, November 8, 1888.

Major Ely McClellan, Surgeon, is relieved from duty at Jefferson Bks., Mo., and will report for duty at Chicago, Ill., as attending surgeon and as examiner of recruits. Par. 13, S. O. 261, A. G. O., Washington, November 8, 1888.

Major John H. Bartholf, Surgeon, is relieved from further duty in the Dept. of Texas, and will report to the commanding officer at Plattsburgh Bks., N. Y. Par. 15, S. O. 26, A. G. O., Washington, November 8, 1888.

Asst. Surgeon W. H. Arthur, upon being relieved by Acting Asst. Surgeon J. L. Ord, will proceed to comply with par 21, S. O. 250, c. s., Hdqrs. of the Army. Par. 5, S. O. 123, Hdqrs. Dept. of Ariz., Los Angeles, Cal., November 5, 1888.

Official List of Changes in the Medical Corps of the U. S. Navy for the Week Ending November 17, 1888.

P. A. Surgeon Robert Swan, ordered before Retiring Board 19th inst.